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ELECTRIC WAVES

BEING

RESEARCHES ON THE PROPAGATION OF ELECTRIC ACTION WITH FINITE VELOCITY THROUGH SPACE

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BY

HEINRICH HERTZ

LATE PROFESSOR OF PHYSICS IN THE UNIVERSITY OF BONN

AUTHORISED ENGLISH TRANSLATION

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WITH A PREFACE

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Dedicated

BY THE AUTHOR

TO

HIS EXCELLENCY

HERMANN VON HELMHOLTZ

WITH THE DEEPEST RESPECT AND GRATITUDE

TRANSLATOR'S NOTE

THE publishers of *Wiedemann's Annalen*, being unable to comply with the numerous applications made for copies of Professor Hertz's researches, invited him to prepare his papers for publication in a collected form. To recast or thoroughly revise them would have been a serious undertaking; and researches are most easily understood when described from the standpoint from which they are undertaken. It was therefore felt best to reprint the separate papers in the form in which they were originally published; but Professor Hertz fortunately decided to supplement the papers by explanatory notes, and to write an Introduction, in which he describes the manner in which the investigations were undertaken, and also discusses their bearing upon electrical theory and the criticisms to which they have been subjected. The collected researches were published early last year under the title, *Untersuchungen über die Ausbreitung der Elektrischen Kraft.*

The book now presented to the English reader is a translation of the German original, with only one or two slight alterations in the notes, and a change, suggested by Lord Kelvin, in the title. I would scarcely have undertaken the translation if I had not been able to rely upon the supervision and kind assistance which Professor Hertz has most freely given, and for which my heartiest thanks are due. I have had the

advantage of revising the proofs with him in Bonn, and now trust that no serious error will have escaped notice.

For advice and help in seeing the book through the press, I am indebted to several friends; but most especially to Dr. Philipp Lenard, *Privat-docent* in the University of Bonn. Dr. Lenard has not only read and revised the translation from start to finish, but has shown as keen an interest in it as if it were his own work. I am very glad to have this opportunity of acknowledging his valuable assistance, and of thanking him most warmly for his kindness.

D. E. JONES.

STAFFORD, *December* 1893.

PREFACE TO THE ENGLISH EDITION

To fully appreciate the work now offered to the English reading public, we must carry our minds back two hundred years to the time when Newton made known to the world the law of universal gravitation. The idea that the sun pulls Jupiter, and Jupiter pulls back against the sun with equal force, and that the sun, earth, moon, and planets all act on one another with mutual attractions seemed to violate the supposed philosophic principle that matter cannot act where it is not. The explanation of the motions of the planets by a mechanism of crystal cycles and epicycles seemed natural and intelligible, and the improvement on this mechanism invented by Descartes in his *vortices* was no doubt quite satisfactory to some of the greatest of Newton's scientific contemporaries. Descartes's doctrine died hard among the mathematicians and philosophers of continental Europe; and for the first quarter of last century belief in universal gravitation was an insularity of our countrymen.

Voltaire, referring to a visit which he made to England in 1727, wrote: "A Frenchman who arrives in London finds a great alteration in philosophy, as in other things. He left the world full; he finds it empty. At Paris you see the univers[e] composed of vortices of subtile matter; at London we s[ee] nothing of the kind. With you it is the pressure of moon which causes the tides of the sea; in England it i[s] sea which gravitates towards the moon. . . . You will [find] also that the sun, which in France has nothing to do w[ith] business, here comes in for a quarter of it. Among tesians all is done by impulsion; with the Newto[nians] done by an attraction of which we know the cause

[1] Whewell's *History of the Inductive Sciences*, vol. ii. p[.]

Indeed, the Newtonian opinions had scarcely any disciples in France till Voltaire asserted their claims on his return from England in 1728. Till then, as he himself says, there were not twenty Newtonians out of England.[1]

In the second quarter of the century sentiment and opinion in France, Germany, Switzerland, and Italy experienced a great change. "The mathematical prize questions proposed by the French Academy naturally brought the two sets of opinions into conflict." A Cartesian memoir of John Bernoulli was the one which gained the prize in 1730. It not infrequently happened that the Academy, as if desirous to show its impartiality, divided the prize between Cartesians and Newtonians. Thus, in 1734, the question being the cause of the inclination of the orbits of the planets, the prize was shared between John Bernoulli, whose memoir was founded on the system of vortices, and his son Daniel, who was a Newtonian. The last act of homage of this kind to the Cartesian system was performed in 1740, when the prize on the question of the tides was distributed between Daniel Bernoulli, Euler, Maclaurin, and Cavallieri; the last of whom had tried to amend and patch up the Cartesian hypothesis on this subject.[2]

On the 4th February 1744 Daniel Bernoulli wrote as follows to Euler: "Uebrigens glaube ich, dass der Aether sowohl *gravis versus solem*, als die Luft versus terram sey, und kann Ihnen nicht bergen, dass ich über diese Puncte ein völliger Newtonianer bin, und verwundere ich mich, dass sie den Principiis Cartesianis so lang adhäriren; es möchte wohl einige Passion vielleicht mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbegreiflich ist, erschaffen, so hat er auch können eine attractionem universalem materiae imprimiren, wenn gleich solche attractio *supra captum* ist, da hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

Here the writer, expressing wonder that Euler had so long ...red to the Cartesian principles, declares himself a thorough-... Newtonian, not merely in respect to gravitation *versus* ..., but in believing that matter may have been created ...ith the law of universal attraction without the aid of ...c medium or mechanism. But in this he was more ...than Newton himself.

[1] ...*ory of the Inductive Sciences*, vol. ii. p. 201. [2] *Ibid.* pp. 198, 199.

Indeed Newton was not a Newtonian, according to Daniel Bernoulli's idea of Newtonianism, for in his letter to Bentley of date 25th February 1692,[1] he wrote: "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it." Thus Newton, in giving out his great law, did not abandon the idea that matter cannot act where it is not. In respect, however, merely of philosophic thought, we must feel that Daniel Bernoulli was right; we can conceive the sun attracting Jupiter, and Jupiter attracting the sun, without any intermediate medium, if they are ordered to do so. But the question remains—Are they so ordered? Nevertheless, I believe all, or nearly all, his scientific contemporaries agreed with Daniel Bernoulli in answering this question affirmatively. Very soon after the middle of the eighteenth century Father Boscovich[2] gave his brilliant doctrine (if infinitely improbable theory) that elastic rigidity of solids, the elasticity of compressible liquids and gases, the attractions of chemical affinity and cohesion, the forces of electricity and magnetism; in short, all the properties of matter except heat, which he attributed to a sulphureous essence, are to be explained by mutual attractions and repulsions, varying solely with distances, between mathematical points endowed also, each of them, with inertia. Before the end of the eighteenth century the idea of action-at-a-distance through absolute vacuum had become so firmly established, and Boscovich's theory so unqualifiedly accepted as a reality, that the idea of gravitational force or electric force or magnetic force being propagated through and by a medium, seemed as wild to the naturalists and mathematicians of one hundred years ago as action-at-a-distance had seemed to Newton and his contemporaries one hundred years earlier. But a retrogression from the eighteenth century school of science set in early in the nineteenth century.

[1] *The Correspondence of Richard Bentley, D.D.*, vol. i. p. 70.

[2] *Theoria Philosophiæ Naturalis redacta ad unicam legem virium existentium auctore P. Rogerio Josepho Boscovich, Societatis Jesu*, fir Vienna, 1758. Second edition, amended and extended by the Author,

Faraday, with his curved lines of electric force, and his dielectric efficiency of air and of liquid and solid insulators, resuscitated the idea of a medium through which, and not only through which but *by* which, forces of attraction or repulsion, seemingly acting at a distance, are transmitted. The long struggle of the first half of the eighteenth century was not merely on the question of a medium to serve for gravific mechanism, but on the correctness of the Newtonian law of gravitation as a matter of fact however explained. The corresponding controversy in the nineteenth century was very short, and it soon became obvious that Faraday's idea of the transmission of electric force by a medium not only did not violate Coulomb's law of relation between force and distance, but that, if real, it must give a thorough explanation of that law.[1] Nevertheless, after Faraday's discovery [2] of the different specific inductive capacities of different insulators, twenty years passed before it was generally accepted in continental Europe. But before his death, in 1867, he had succeeded in inspiring the rising generation of the scientific world with something approaching to faith that electric force is transmitted by a medium called ether, of which, as had been believed by the whole scientific world for forty years, light and radiant heat are transverse vibrations. Faraday himself did not rest with this theory for electricity alone. The very last time I saw him at work in the Royal Institution was in an underground cellar, which he had chosen for freedom from disturbance; and he was arranging experiments to test the time of propagation of magnetic force from an electromagnet through a distance of many yards of air to a fine steel needle polished to reflect light; but no result came from those experiments. About the same time or soon after, certainly not long before the end of his working time, he was engaged (I believe at the shot tower near Waterloo Bridge on the Surrey side) in efforts to discover relations between gravity and magnetism, which also led to no result.

Absolutely nothing has hitherto been done for gravity [illegible]er by experiment or observation towards deciding between

[illegible]*ctrostatics and Magnetism*, Sir W. Thomson, Arts. I. (1842) and II. [illegible]rticularly § 25 of Art. II.

[illegible] *Experimental Researches*, 1161-1306.

Newton and Bernoulli, as to the question of its propagation through a medium, and up to the present time we have no light, even so much as to point a way for investigation in that direction. But for electricity and magnetism Faraday's anticipations and Clerk-Maxwell's splendidly developed theory have been established on the sure basis of experiment by Hertz's work, of which his own most interesting account is now presented to the English reader by his translator, Professor D. E. Jones. It is interesting to know, as Hertz explains in his introduction, and it is very important in respect to the experimental demonstration of magnetic waves to which he was led, that he began his electric researches in a problem happily put before him thirteen years ago by Professor von Helmholtz, of which the object was to find by experiment some relation between electromagnetic forces and dielectric polarisation of insulators, without, in the first place, any idea of discovering a progressive propagation of those forces through space.

It was by sheer perseverance in philosophical experimenting that Hertz was led to discover (VII., p. 107 below) a finite velocity of propagation of electromagnetic action, and then to pass on to electromagnetic waves in air and their reflection (VIII.), and to be able to say, as he says in a short reviewing sentence at the end of VIII.: "Certainly it is a fascinating idea that the processes in air which we have been investigating, represent to us on a million-fold larger scale the same processes which go on in the neighbourhood of a Fresnel mirror or between the glass plates used for exhibiting Newton's rings."

Professor Oliver Lodge has done well, in connection with Hertz's work, to call attention[1] to old experiments, and ideas taken from them, by Joseph Henry, which came more nearly to an experimental demonstration of electromagnetic waves than anything that had been done previously. Indeed Henry, after describing experiments showing powerful enough induction due to a single spark from the prime conductor of an electric machine to magnetise steel needles at a distance of 30 feet in a cellar beneath with two floors and ceilings intervening, s that he is "disposed to adopt the hypothesis of an elect plenum," and concludes with a short reviewing sentenc

[1] *Modern Views of Electricity*, pp. 369-372.

may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light."

Professor Oliver Lodge himself did admirable work in his investigations regarding lightning rods,[1] coming very near to experimental demonstration of electromagnetic waves; and he drew important lessons regarding "electrical surgings" in an insulated bar of metal "induced by Maxwell's and Heaviside's electromagnetic waves," and many other corresponding phenomena manifested both in ingenious and excellent experiments devised by himself and in natural effects of lightning.

Of electrical surgings or waves in a short insulated wire, and of interference between ordinary and reflected waves, and positive electricity appearing where negative might have been expected, we hear first it seems in Herr von Bezold's "Researches on the Electric Discharge" (1870), which Hertz gives as the Third Paper in the present series, with interesting and ample recognition of its importance in relation to his own great work.

Readers of the present volume will, I am sure, be pleased if I call their attention to two papers by Prof. G. F. Fitzgerald which I heard myself at the meeting of the British Association at Southport in 1883. One of them is entitled, "On a Method of producing Electromagnetic Disturbances of comparatively Short Wave-lengths." The paper itself is not long, and I quote it here in full, as it appeared in the *Report of the British Association*, 1883: "This is by utilising the alternating currents produced when an accumulator is discharged through a small resistance. It is possible to produce waves of as little as two metres wave-length, or even less." This was a brilliant and useful suggestion. Hertz, not knowing of it, used the method; and, making as little as possible of the "accumulator," got waves of as little as *twenty-four centimetres* wave-length in many of his fundamental experiments. The title alone of the other paper, "On the Energy lost by Radiation from Alternating Currents," is in itself a valuable lesson [illegible] the electromagnetic theory of light, or the undulatory theory [illegible]agnetic disturbance. The reader of the present volume

[1] *[illegible]ghtning Conductors and Lightning Guards*, Oliver J. Lodge, D.Sc., F.R.S. [illegible]r and Co.

will be interested in comparing it with the title of Hertz's Eleventh Paper; but I cannot refer to this paper without expressing the admiration and delight with which I see the words "rectilinear propagation," "polarisation," "reflection," "refraction," appearing in it as sub-titles.

During the fifty-six years which have passed since Faraday first offended physical mathematicians with his curved lines of force, many workers and many thinkers have helped to build up the nineteenth-century school of *plenum*, one ether for light, heat, electricity, magnetism; and the German and English volumes containing Hertz's electrical papers, given to the world in the last decade of the century, will be a permanent monument of the splendid consummation now realised.

KELVIN.

CONTENTS

		PAGE
I.	INTRODUCTION—	
	A. EXPERIMENTAL	1
	B. THEORETICAL	20
II.	ON VERY RAPID ELECTRIC OSCILLATIONS	29
III.	FROM HERR W. VON BEZOLD'S PAPER: "RESEARCHES ON THE ELECTRIC DISCHARGE—PRELIMINARY COMMUNICATION" .	54
IV.	ON AN EFFECT OF ULTRA-VIOLET LIGHT UPON THE ELECTRIC DISCHARGE	63
V.	ON THE ACTION OF A RECTILINEAR ELECTRIC OSCILLATION UPON A NEIGHBOURING CIRCUIT	80
VI.	ON ELECTROMAGNETIC EFFECTS PRODUCED BY ELECTRICAL DISTURBANCES IN INSULATORS	95
VII.	ON THE FINITE VELOCITY OF PROPAGATION OF ELECTROMAGNETIC ACTIONS	107
VIII.	ON ELECTROMAGNETIC WAVES IN AIR AND THEIR REFLECTION .	124
IX.	THE FORCES OF ELECTRIC OSCILLATIONS, TREATED ACCORDING TO MAXWELL'S THEORY	137
X.	ON THE PROPAGATION OF ELECTRIC WAVES BY MEANS OF WIRES	160
XI.	ON ELECTRIC RADIATION	172
XII.	ON THE MECHANICAL ACTION OF ELECTRIC WAVES IN WIRES .	186
XIII.	ON THE FUNDAMENTAL EQUATIONS OF ELECTROMAGNETICS FOR BODIES AT REST	195
XIV.	ON THE FUNDAMENTAL EQUATIONS OF ELECTROMAGNETICS FOR BODIES IN MOTION	241
	SUPPLEMENTARY NOTES	269
	INDEX TO NAMES	279

I

INTRODUCTION

A. Experimental

I HAVE often been asked how I was first led to carry out the experiments which are described in the following pages. The general inducement was this. In the year 1879 the Berlin Academy of Science had offered a prize for a research on the following problem:—To establish experimentally any relation between electromagnetic forces and the dielectric polarisation of insulators—that is to say, either an electromagnetic force exerted by polarisations in non-conductors, or the polarisation of a non-conductor as an effect of electromagnetic induction. As I was at that time engaged upon electromagnetic researches at the Physical Institute in Berlin, Herr von Helmholtz drew my attention to this problem, and promised that I should have the assistance of the Institute in case I decided to take up the work. I reflected on the problem, and considered what results might be expected under favourable conditions by using the oscillations of Leyden jars or of open induction-coils. The conclusion at which I arrived was certainly not what I had wished for; it appeared that any decided effect could scarcely be hoped for, but only an action lying just within the limits of observation. I therefore gave up the idea of working at the problem; nor am I aware that it has been attacked by anybody else. But in spite of having abandoned the solution at that time, I still felt ambitious to discover it by some other method; and my interest in everything connected with electric oscillations had become keener. It was scarcely possible that I should overlook any new form

of such oscillations, in case a happy chance should bring such within my notice.

Such a chance occurred to me in the spring of 1886, and brought with it the special inducement to take up the following researches. In the collection of physical instruments at the Technical High School at Karlsruhe (where these researches were carried out), I had found and used for lecture purposes a pair of so-called Riess or Knochenhauer spirals. I had been surprised to find that it was not necessary to discharge large batteries through one of these spirals in order to obtain sparks in the other; that small Leyden jars amply sufficed for this purpose, and that even the discharge of a small induction-coil would do, provided it had to spring across a spark-gap. In altering the conditions I came upon the phenomenon of side-sparks which formed the starting-point of the following research. At first I thought the electrical disturbances would be too turbulent and irregular to be of any further use; but when I had discovered the existence of a neutral point in the middle of a side-conductor, and therefore of a clear and orderly phenomenon, I felt convinced that the problem of the Berlin Academy was now capable of solution. My ambition at the time did not go further than this. My conviction was naturally strengthened by finding that the oscillations with which I had to deal were regular. The first of the papers here republished ("On very Rapid Electric Oscillations") gives, generally in the actual order of time, the course of the investigation as far as it was carried out up to the end of the year 1886 and the beginning of 1887.

While this paper was in the press I learned that its contents were not as new as I had believed them to be. The Geographical Congress of April 1887 brought Herr W. von Bezold to Karlsruhe and into my laboratory. I spoke to him about my experiments; he replied that years ago he had observed similar phenomena, and he drew my attention to his "Researches on the Electric Discharge," in vol. cxl. of Poggendorff's *Annalen*. This paper had entirely escaped me, inasmuch as its external appearance seemed to indicate that it related to matters quite other than electric oscillations, namely, Lichtenberg figures; indeed, it does not appear to have attracted such attention as the importance of its contents merited. In an appendix to

my paper I acknowledged Herr von Bezold's prior claim to a whole series of observations. In place of this appendix, I here, with Herr von Bezold's kind consent, include as the second of these papers that part of his communication which is of the most immediate interest in the present connection. It may now well be asked with surprise how it was possible that results so important and so definitely stated should have exercised no greater influence upon the progress of science? Perhaps the fact that Herr von Bezold described his communication as a preliminary one may have something to do with this.

I may here be permitted to record the good work done by two English colleagues who at the same time as myself were striving towards the same end. In the same year in which I carried out the above research, Professor Oliver Lodge, in Liverpool, investigated the theory of the lightning-conductor, and in connection with this carried out a series of experiments on the discharge of small condensers which led him on to the observation of oscillations and waves in wires. Inasmuch as he entirely accepted Maxwell's views, and eagerly strove to verify them, there can scarcely be any doubt that if I had not anticipated him he would also have succeeded in observing waves in air, and thus also in proving the propagation with time of electric force. Professor Fitzgerald, in Dublin, had some years before endeavoured to predict, with the aid of theory, the possibility of such waves, and to discover the conditions for producing them. My own experiments were not influenced by the researches of these physicists, for I only knew of them subsequently. Nor, indeed, do I believe that it would have been possible to arrive at a knowledge of these phenomena by the aid of theory alone. For their appearance upon the scene of our experiments depends not only upon their theoretical possibility, but also upon a special and surprising property of the electric spark which could not be foreseen by any theory.

By means of the experiments already mentioned I had succeeded in obtaining a method of exciting more rapid electric disturbances than were hitherto at the disposal of physicists. But before I could proceed to apply this method

to the examination of the behaviour of insulators, I had to finish with another investigation. Soon after starting the experiments I had been struck by a noteworthy reciprocal action between simultaneous electrical sparks. I had no intention of allowing this phenomenon to distract my attention from the main object which I had in view; but it occurred in such a definite and perplexing way that I could not altogether neglect it. For some time, indeed, I was in doubt whether I had not before me an altogether new form of electrical action-at-a-distance. The supposition that the action was due to light seemed to be excluded by the fact that glass plates cut it off; and naturally it was some time before I came to experiment with plates of rock-crystal. As soon as I knew for certain that I was only dealing with an effect of ultra-violet light, I put aside this investigation so as to direct my attention once more to the main question. Inasmuch as a certain acquaintance with the phenomenon is required in investigating the oscillations, I have reprinted the communication relating to it ("On an Effect of Ultra-Violet Light upon the Electric Discharge") as the fourth of these papers. A number of investigators, more especially Herren Righi, Hallwachs, and Elster and Geitel, have helped to make our knowledge of the phenomenon more accurate; nevertheless, the mechanics of it have not yet been completely disclosed to our understanding.

The summer of 1887 was spent in fruitless endeavours to establish the electromagnetic influence of insulators by the aid of the new class of oscillations. The simplest method consisted in determining the effect of dielectrics upon the position of the neutral point of a side-circuit. But in that case I should have had to include the electrostatic forces in the bargain, whereas the problem consisted precisely in investigating the electromagnetic induction alone. The plan which I adopted was the following:—The primary conductor[1] had the form shown in Fig. 1; between the plates A and A' at its ends was introduced a block BB of sulphur or paraffin, and this was then quickly removed. I placed the secondary conductor C in the same position, with respect to the primary, as before (the only position which I had taken

[1] The reader is assumed to be already acquainted with the papers referred to.

into consideration), and expected that when the block was in place very strong sparks would appear in the secondary, and that when the block was removed there would only be feeble sparks. This latter expectation was based upon the supposition that the electrostatic forces could in no case induce a spark in the almost closed circuit C, for since these forces have a potential, it follows that their integral over a nearly closed circuit is vanishingly small. Thus in the absence of the insulator we should only have to consider the inductive effect of the more distant wire $a\,b$. The experiment was frustrated by the invariable occurrence of strong sparking in the secondary conductor, so that the moderate strengthening or weakening effect which the insulator must exert did not make itself felt. It only gradually became clear to me that the law which I had assumed as the basis of my experiment did not apply here; that on account of the rapidity of the motion even forces which possess a potential are able to induce sparks in the nearly closed conductor; and, in general, that the greatest care has to be observed in applying here the general ideas and laws which form the basis of the usual electrical theories. These laws all relate to statical or stationary states; whereas here I had truly before me a variable state. I perceived that I had in a sense attacked the problem too directly. There was yet an infinite number of other positions of the secondary with respect to the primary conductor, and among these there might well be some more favourable for my purpose. These various positions had first to be examined. Thus I came to discover the phenomena which are described in the fifth paper ("On the Action of a Rectilinear Electric Oscillation upon a Neighbouring Circuit"), and which surprised me by their variety and regularity. The finding out and unravelling of these extremely orderly phenomena gave me peculiar pleasure. The paper certainly does not include all the discoverable details; whoever may extend

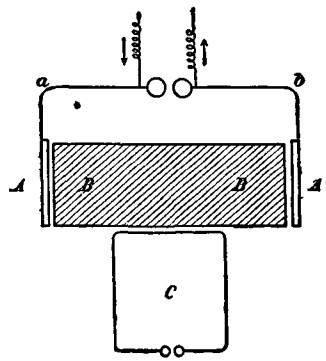


Fig. 1.

the experiments to various other forms of conductor will find that the task is not an ungrateful one. The observations at greater distances are also probably very inaccurate, for they are affected by the disturbing influence of reflections which were not at that time suspected. What especially surprised me was the continual increase of the distance up to which I could perceive the action; up to that time the common view was that electric forces decreased according to the Newtonian law, and therefore rapidly tended to zero as the distance increased.

Now during the course of this investigation I had made sure of other positions of the secondary conductor in which it was possible, by bringing an insulator near, to cause the appearance or disappearance of sparks, instead of simply altering their size. The problem which I was investigating was now solved directly in the manner described in the sixth paper ("On Electromagnetic Effects produced by Electrical Disturbances in Insulators"). On 10th November 1887 I was able to report the successful issue of the work to the Berlin Academy.

The particular problem of the Academy which had been my guide thus far was evidently propounded at the time by Herr von Helmholtz in the following connection:—If we start from the electromagnetic laws which in 1879 enjoyed universal recognition, and make certain further assumptions, we arrive at the equations of Maxwell's theory which at that time (in Germany) were by no means universally recognised. These assumptions are: first, that changes of dielectric polarisation in non-conductors produce the same electromagnetic forces as do the currents which are equivalent to them; secondly, that electromagnetic forces as well as electrostatic are able to produce dielectric polarisations; thirdly, that in all these respects air and empty space behave like all other dielectrics. In the latter part of his paper ("On the Equations of Motion of Electricity for conducting Bodies at Rest"),[1] von Helmholtz has deduced Maxwell's equations from the older views and from hypotheses which are equivalent to those just stated. The problem of proving all three hypotheses, and thereby establishing the correctness of the whole of

[1] v. Helmholtz, *Ges. Abhandl.* 1, p. 545.

Maxwell's theory, appeared to be an unreasonable demand; the Academy, therefore, contented itself with requiring a confirmation of one of the first two.

The first assumption was now shown to be correct. I thought for some time of attacking the second. To test it appeared by no means impossible; and for this purpose I cast closed rings of paraffin. But while I was at work it struck me that the centre of interest in the new theory did not lie in the consequences of the first two hypotheses. If it were shown that these were correct for any given insulator, it would follow that waves of the kind expected by Maxwell could be propagated in this insulator, with a finite velocity which might perhaps differ widely from that of light. I felt that the third hypothesis contained the gist and special significance of Faraday's, and therefore of Maxwell's, view, and that it would thus be a more worthy goal for me to aim at. I saw no way of testing separately the first and the second hypotheses for air;[1] but both hypotheses would be proved simultaneously if one could succeed in demonstrating in air a finite rate of propagation and waves. Certainly some of the first experiments in this direction failed; these are described in the paper referred to, and they were carried out at short distances. But in the meantime I had succeeded in detecting the inductive action at distances up to 12 metres. Within this distance the phase of the motion must have been reversed more than once; and now it only remained to detect and prove this reversal. Thus the scheme was conceived which was carried out as described in the research "On the Finite Velocity of Propagation of Electromagnetic Actions." The first step that had to be taken was easy. In straight stretched wires surprisingly distinct stationary waves were produced with nodes and antinodes, and by means of these it was possible to determine the wave-length and the change of phase along the wire. Nor was there any greater difficulty in producing interference between the action which had travelled along the wire and that which had travelled through the air, and thus in comparing their phases. Now if both actions

[1] The expressions air (*Luftraum*) and empty space (*leerer Raum*) are here used as synonymous, inasmuch as the influence of the air itself in these experiments is negligible.

were propagated, as I expected, with one and the same finite velocity, they must at all distances interfere with the same phase. A simple qualitative experiment which, with the experience I had now gained, could be finished within an hour, must decide this question and lead at once to the goal. But when I had carefully set up the apparatus and carried out the experiment, I found that the phase of the interference was obviously different at different distances, and that the alternation was such as would correspond to an infinite rate of propagation in air. Disheartened, I gave up experimenting. Some weeks passed before I began again. I reflected that it would be quite as important to find out that electric force was propagated with an infinite velocity, and that Maxwell's theory was false, as it would be, on the other hand, to prove that this theory was correct, provided only that the result arrived at should be definite and certain. I therefore confirmed with the greatest care, and without heeding what the outcome might be, the phenomena observed: the conclusions arrived at are given in the paper. When I then proceeded to consider more closely these results, I saw that the sequence of the interferences could not be harmonised with the assumption of an infinite rate of propagation; that it was necessary to assume that the velocity was finite, but greater than that in the wire. As shown in the paper, I endeavoured to bring into harmony the various possibilities; and although the difference in the velocities appeared to me to be somewhat improbable, I could see no reason for mistrusting the experiments. And it was not by any means impossible that the motion in the wire might be retarded by some unknown causes, as, for example, by an essential inertia of the free electricity.

I have entered into these details here in order that the reader may be convinced that my desire has not been simply to establish a preconceived idea in the most convenient way by a suitable interpretation of the experiments. On the contrary, I have carried out with the greatest possible care these experiments (by no means easy ones), although they were in opposition to my preconceived views. And yet, although I may have been lucky elsewhere, in this research I have been decidedly unlucky. For instead of reaching the right goal with little effort, as a properly devised plan might

have enabled me to do, I seem to have taken great pains, and to have fallen into error after all.

In the first place, the research is disfigured by an error of calculation. The time of oscillation is overestimated in the ratio of $\sqrt{2}:1$. M. Poincaré first drew attention to this error.[1] As a matter of fact, this error affects the form of the research more than the substance of it. My reliance on the correctness of the calculation was mainly due to its supposed accordance with the experiments of Siemens and Fizeau and with my own.[2] If I had used the correct value for the capacity, and so found out the discrepancy between calculation and experiment, I would have placed less reliance on the calculation; the investigation would have been somewhat altered in form, but the subject-matter would have remained unaltered.

In the second place (and this is the more important point), one of the principal conclusions of the investigation can scarcely be regarded as correct—namely, that the velocities in air and in the wire are different. Such further knowledge as has been gained respecting waves in wires, instead of confirming this result, tends to make it more and more improbable. It now seems fairly certain that if the experiment had been carried out quite correctly, and without any disturbing causes, it would have given almost exactly the result which I expected at the start. There is no doubt that the phase of the interference must have changed sign once (and this I had not expected beforehand); but the interference should have exhibited no second change of sign; and yet the experiments without exception pointed to this. It is not easy to point to any disturbing cause which could imitate in such a deceptive way the effect of a difference in velocity; but there is no reason why we should not admit the possibility of such a deception. While performing the experiments, I never in the least suspected that they might be affected by the neighbouring walls. I remember that the wire along which the waves travelled was carried past an iron stove, and only 1·5 metres from it. A disturbance caused in this way, and always acting at the same point, might have given rise to the second change

[1] H. Poincaré, *Comptes Rendus*, 111, p. 322.

[2] See the remark at the end of the second part of the paper (p. 114).

of phase of the interference. However this may be, I should like to express a hope that these experiments may be repeated by some other observer under the most favourable conditions possible, *i.e.* in a room as large as possible. If the plan of the experiment is correct, as I think it is, then it must, whenever properly carried out, give the result which it should at first have given; it would then prove without measurement the finite velocity of propagation of the waves in air, and at the same time the equality between this and the velocity of the waves in the wire.

I might also mention here some further considerations which at that time strengthened my conviction that the waves in the wire suffered a retardation. If the waves in the wire run along at the same speed as waves in air, then the lines of electric forces must be perpendicular to the wire. Thus a straight wire traversed by waves cannot exert any inductive action upon a neighbouring parallel wire. But I found that there was such an action, even though it was only a weak one. I concluded that the lines of force were not parallel to the wire, and that the velocity of the waves was not the same as that of light. Further, if the lines of force are perpendicular to the wire, it can be shown by a simple calculation that the energy propagated by a wave in a single wire becomes logarithmically infinite. I therefore concluded that such a wave was *a priori* impossible. Lastly, it seemed to me that it could have no effect upon the rate of propagation in a straight conductor, whether that conductor was a smooth wire or a wire with side projections, or a crooked wire, or a spiral wire with small convolutions, provided always that these deviations from the straight line were small compared with the wave-length, and that their resistance did not come into consideration. But now I found that all these alterations produced a very noticeable effect upon the velocity. Hence I concluded that here again there was some obscure cause at work which caused a retardation, and which would also make itself felt in simple smooth wires. At the present moment these and other reasons do not appear to me to be of decisive weight; but at that time they so far satisfied me that I asserted without any reserve that there was a difference between the velocities, and regarded this decision as one of the most interesting of my

experimental results. Soon I was to discover what appeared to be a confirmation of my opinion; and at that time it was very welcome.

While investigating the action of my primary oscillation at great distances, I came across something like a formation of shadows behind conducting masses, and this did not strike me as being very surprising. Somewhat later on I thought that I noticed a peculiar reinforcement of the action in front of such shadow-forming masses, and of the walls of the room. At first it occurred to me that this reinforcement might arise from a kind of reflection of the electric force from the conducting masses; but although I was familiar with the conceptions of Maxwell's theory, this idea appeared to me to be almost inadmissible—so utterly was it at variance with the conceptions then current as to the nature of an electric force. But when I had established with certainty the existence of actual waves, I returned to the mode of explanation which I had at first abandoned, and so arrived at the phenomena which are described in the paper "On Electromagnetic Waves in Air, and their Reflection." No objection can be urged against the qualitative part of this research—the experiments have been frequently repeated and confirmed. But the part of the research which relates to the measurements is doubtful, inasmuch as it also leads to the very unlikely result that the velocity in air is considerably greater than that of waves in wires. Assuming that this result is incorrect, how are we to explain the error which has crept in? Certainly it is not due to simple inaccuracy of observation. The error of observation may perhaps be about a decimetre, but certainly not a metre. I can only here attribute the mistake in a general sense to the special conditions of resonance of the room used. The vibrations natural to it may possibly have been aroused, and I may have observed the nodes of such a vibration when I thought that I was observing the nodes of the waves of the primary conductor. There was certainly a substantial difference between the distances of the nodes in air which I measured and the wave-lengths in the wire. I specially directed my attention to the question whether or not such a difference existed. As far as any exact accordance with the first series of experi-

ments is concerned, I freely allow that in the interpretation of the experimental results I may have allowed myself to be influenced by a desire to establish an accordance between the two sets of measurements. I put back the first node a certain distance behind the wall, and an exact control of the amount of this cannot be deduced from the experiments. If I had wished to combine the experiments otherwise, I might indeed have been able to calculate a ratio of the velocities which would come out nearer to unity; but I certainly could not infer from them that the velocities were equal.

Now, if the experiments which I made at that time all agree in pointing to a difference between the velocities, it will naturally be asked what reasons now induce me to allow that there may have been unknown sources of error in the experiments, rather than to abide by the statement made as to the difference of velocities. Is it the objection which has been raised in several quarters as to the want of accord between the results and the theory? Certainly not. The theory was known to me at the time; and furthermore, it must be subordinated to the experiments. Is it the experiment in this connection made by Herr Lecher?[1] This, too, I must deny, although I fully recognise the value of the work which Herr Lecher has done in this direction. In working out his results Herr Lecher assumes that the calculation is correct, and therefore in a certain sense that the theory itself is correct.[2] Is it then the results of MM. Sarasin and de la Rive,[3] who carefully repeated the experiments and arrived at conclusions which were completely in accord with the theory? In a certain sense, yes; in another sense, no. The Genevan physicists worked in a much smaller room than my own; the greatest distance of which they could avail themselves was only 10 metres, and the waves could not develop quite freely even up to this distance. Their mirror was only 2·8 metres high. Care in carrying out the observations cannot compensate for the unfavourable nature of the room. In my experiments, on the other hand, the waves had perfectly free play up to 15

[1] E. Lecher, "Eine Studie über elektrische Resonanzerscheinungen," *Wied. Ann.* **41**, p. 850.

[2] The same remark holds good for the work recently published by M. Blondlot, *C. R.* **113**, p. 628 (cp. Note 15 at end of book).

[3] E. Sarasin and L. de la Rive, *Comptes Rendus*, **112**, p. 658.

metres. My mirror was 4 metres high. If the decision rested simply and solely with the experiments, I could not attribute greater weight to those of MM. Sarasin and de la Rive than to my own.[1] So far, then, I again say no. But certainly the Genevan experiments show that my experiments are subject to local variations; they show that the phenomena are different if the reflecting walls and the rooms are different, and also that under certain conditions the wave-lengths have the values required by theory. But if the experiments furnish information which is ambiguous and contradictory, they obviously contain sources of error which are not understood; and hence they cannot be brought forward as arguments against a theory which is supported by so many reasons based on probability. Thus the Genevan experiments deprive my own of their force, and so far they restore the balance of probability to the theoretical side.

Still, I must acknowledge that the reasons which decided me were of a more indirect kind. When I first thought that I had found a retardation of waves in the wires, I hoped soon to discover the cause of this retardation, and to find some gradual change in its value. This hope has not been realised. I found no such change, and, as my experience increased, instead of coming across an explanation, I met with increasing discrepancies, until these at last appeared to me to be insoluble, and I had to give up all hope of proving the correctness of my first observation. My own discovery, that for short waves the difference between the velocities very nearly disappears, tended in the same direction. Before one of my scientific colleagues had attacked this question, I had stated my opinion in the following words:[2]—"Thus I found that for long waves the wave-length is greater in air than in wires, whereas for short waves both appear to be practically equal. This result is so surprising that we cannot regard it as certain. The decision must be reserved until further experiments are made." The only experiments of the kind referred to that have hitherto been made are those of MM. Sarasin and de la Rive; and inasmuch as these were carried

[1] Mr. Trouton, in a room of which the dimensions are not exactly given, found, like myself, that the wave-length of my primary conductor in air was about 10 metres.—*Nature*, **39**, p. 391.

[2] *Archives de Genève* (3), **21**, p. 302.

out in small rooms, they may more properly be regarded as a confirmation of the second part of my statement than as a refutation of the first part. Decisive experiments for long waves seem to me to be still wanting.[1] I have little doubt that they will decide in favour of equal velocities in all cases.

The reader may, perhaps, ask why I have not endeavoured to settle the doubtful point myself by repeating the experiments. I have indeed repeated the experiments, but have only found, as might be expected, that a simple repetition under the same conditions cannot remove the doubt, but rather increases it. A definite decision can only be arrived at by experiments carried out under more favourable conditions. More favourable conditions here mean larger rooms, and such were not at my disposal. I again emphasise the statement that care in making the observations cannot make up for want of space. If the long waves cannot develop, they clearly cannot be observed.

The experiments hitherto described on the reflection of waves were finished in March 1888. In the same month I attempted, by means of reflection at a curved surface, to prevent the dispersion of the action. For my large oscillator I built a concave parabolic mirror of 2 metres aperture and 4 metres high. Contrary to my expectation I found that the action was considerably weakened. The large mirror acted like a protecting screen surrounding the oscillator. I concluded that the wave-length of the oscillation was too large in comparison with the focal length of the mirror. A moderate reduction in the size of the primary conductor did not improve the result. I therefore tried to work with a conductor which was geometrically similar to the larger one, but smaller in the proportion of 10 : 1. Perhaps I did not persevere sufficiently in this attempt; at any rate I entirely failed at that time to pro-

[1] Since the above was written, the wish expressed has been amply satisfied by the experiments which MM. Sarasin and de la Rive have carried out in the great hall of the Rhone waterworks at Geneva (see *Archives de Genève*, **29**, pp. 358 and 441). These experiments have proved the equality of the velocity in air and in wires, and have thus established the full agreement between experiment and theory. I consider these experiments to be conclusive, and submit to them now with as much readiness as I then felt hesitation in submitting to experiments which were not superior to my own. I gladly avail myself of the opportunity of thanking MM. Sarasin and de la Rive for the great kindness and goodwill which they invariably exhibited in the whole controversy—a controversy which has now been decided entirely in their own favour.

duce and observe such short oscillations, and I abandoned these experiments in order to turn my attention to other questions.

In the first place, it was important to devise a clearer theoretical treatment of the experiments. In the researches to which I have hitherto referred, the experiments were interpreted from the standpoint which I took up through studying von Helmholtz's papers.[1] In these papers Herr v. Helmholtz distinguishes between two forms of electric force—the electromagnetic and the electrostatic—to which, until the contrary is proved by experience, two different velocities are attributed. An interpretation of the experiments from this point of view could certainly not be incorrect, but it might perhaps be unnecessarily complicated. In a special limiting case Helmholtz's theory becomes considerably simplified, and its equations in this case become the same as those of Maxwell's theory; only one form of the force remains, and this is propagated with the velocity of light. I had to try whether the experiments would not agree with these much simpler assumptions of Maxwell's theory. The attempt was successful. The results of the calculation are given in the paper on "The Forces of Electric Oscillations, treated according to Maxwell's Theory." That part of the research which relates to interference between waves in air and in wires could clearly be adapted without difficulty to any other form of such interference which might result from more complete experiments.

Side by side with the theoretical discussions I continued the experimental work, directing the latter again more to waves in wires. In doing so, my primary object was to find out the cause of the supposed retardation of these waves. Secondly, I wished to test the correctness of the view according to which the seat and field of action of the waves is not in the interior of the conductor, but rather in the surrounding space. I now made the waves travel in the interspace between two wires, between two plates, and in tubular spaces, instead of along a single wire; in various interposed insulators instead of in different metals. The research on "The Propagation of Electric Waves by Means of Wires" was, for the most part, carried out in the summer of 1888, although it was only completed and published later on.

[1] v. Helmholtz, *Ges. Abhandl.* 1, p. 545.

For in the autumn a singular phenomenon attracted my attention away from the experiments with wires. For the investigation of waves in the narrow interspace between two wires I was using resonators of small external dimensions, and was engaged in tuning these. I found that I obtained distinct nodes at the end of the wires even when I used resonators which were much too small. Even when I diminished the size of the circles to a few centimetres diameter, I still obtained nodes; these were situated at a small distance from the end of the wires, and I could observe half wave-lengths as small as 12 cm. Thus chance brought me on to the track, hitherto undiscovered, of the short waves. I at once followed up this track, and soon succeeded in finding a form of the primary conductor which could be used with the small resonators.

I paid no special attention to the phenomenon which led me back to the observation of short waves; and, as no suitable occasion arose for doing so, I have not mentioned it in my papers. Clearly it was a special case of the same phenomenon which was later on discovered by MM. Sarasin and de la Rive,[1] called by the name of "Multiple Resonance," and explained by saying that the primary conductor did not possess any definite period of oscillation, but that it performed simultaneously all possible oscillations lying within wide limits. If I paid little attention myself to this phenomenon, it was partly because I was soon led on to other researches. It arose no less from the fact that I had from the start conceived an interpretation of the phenomenon which lent much less interest to it than the interpretation given by MM. Sarasin and de la Rive. I regarded the phenomenon as a consequence of the rapid damping of the primary oscillation—a necessary consequence, and one which could be foreseen. M. Sarasin was good enough to communicate at once to me the results of his research, and I told him my doubts as to his explanation of the phenomenon, and gave him my own explanation of it; but although he received my explanation with the readiest goodwill, we did not succeed in coming to a common understanding as to the interpretation of the experiment. With M. H. Poincaré such an understanding was secured at once; he had formed a conception of the

[1] E. Sarasin and L. de la Rive, *Arch. de Genève* (3), **23**, p. 113, 1890.

phenomenon which was practically identical with my own, and had communicated it to me in a letter. This conception he has worked out mathematically, and published in his book *Electricité et Optique*.[1] Herr V. Bjerknes has worked out the mathematical developments simultaneously and independently.[2] That the explanation given by MM. Poincaré and Bjerknes is not only a possible one, but is the only possible one, appears to me to be proved by an investigation by Herr Bjerknes,[3] which has appeared recently, and which makes it certain that the vibration of the primary conductor is, at any rate to a first approximation, a uniformly damped sine-wave of determinate period. Hence the careful investigations of MM. Sarasin and de la Rive are of great value in completing our knowledge of this part of the work, but they in no way contradict any statement made by me. The authors themselves regard their experiments in this light. Nevertheless, these experiments gave occasion to an adverse criticism of my work from a distinguished French physicist who had not, however, repeated the experiments himself. I hope it will now be allowed that there was no cause for such a criticism.[4]

I may be permitted to take this opportunity of referring to the doubts which have recently been raised by Herrn Hagenbach and Zehnder as to what my experiments really prove.[5] Perhaps I ought not yet to consider their work as being completed. The authors reserve to themselves the right of returning to the explanation of resonance and the formation of nodes and antinodes in my experiments. But it is just precisely upon these phenomena that my experiments, and the whole interpretation of them, rest.

After I had succeeded (as already described) in observing very short waves, I chose waves about 30 cm. long, and repeated first of all the earlier experiments with these. I now found, contrary to my expectation, that these short waves travelled along wires with very nearly the same velocity as in air. As it was easy to procure free play for such short waves, no doubt could arise in this case as to the correctness of the

[1] H. Poincaré, *Electricité et Optique*, **2**, p. 249.
[2] V. Bjerknes, *Wied. Ann.* **44**, p. 92, 1891.
[3] *Ibid.* **45**, p. 513, 1891.
[4] Cornu, *Comptes Rendus*, **110**, p. 72, 1890.
[5] E. Hagenbach and L. Zehnder, *Wied. Ann.* **43**, p. 610, 1891.

results. After I had become quite used to managing these short waves, I returned to the experiment with the concave reflector. The large old reflector was no longer at my disposal, so I had a smaller one made, about 2 metres high and a little more than 1 metre in aperture. It worked so remarkably well that, directly after the first trial, I ordered not only a second concave reflector, but also a plane reflecting surface and a large prism. The experiments which are described in the paper "On Electric Radiation" now followed each other in rapid succession, and without difficulty; they had been considered and prepared long beforehand, with the exception of the polarisation-experiments, which only occurred to me during the progress of the work. These experiments with concave mirrors soon attracted attention; they have frequently been repeated and confirmed. The approval with which they have been received has far exceeded my expectation.[1] A considerable part of this approval was due to reasons of a philosophic nature. The old question as to the possibility and nature of forces acting at a distance was again raised. The preponderance of such forces in theory has long been sanctioned by science, but has always been accepted with reluctance by ordinary common sense; in the domain of electricity these forces now appeared to be dethroned from their position by simple and striking experiments.

Though in the last-mentioned experiments my research had, in a certain sense, come to its natural end, I still felt that there was one thing wanting. The experiments related only to the propagation of the electric force. It was desirable to show that the magnetic force was also propagated with a finite velocity. According to theory it was not necessary for this purpose to produce special magnetic waves; the electric waves should at the same time be waves of magnetic force; the only important thing was to really detect in these waves the magnetic force in the presence of the electric force. I hoped that it would be possible to do this by observing the mechanical forces which the waves exerted upon ring-shaped conductors. So experiments were planned which (for other reasons) were only

[1] These experiments gave occasion to the lecture "On the Relations between Light and Electricity," which I delivered to the *Naturforscherversammlung* at Heidelberg in 1889, and in which I gave a general account of my experiments in a popular form (published by E. Strauss, Bonn).

carried out later on, and then incompletely; these are described in the last experimental research "On the Mechanical Action of Electric Waves in Wires."

Casting now a glance backwards we see that by the experiments above sketched the propagation in time of a supposed action-at-a-distance is for the first time proved. This fact forms the philosophic result of the experiments; and, indeed, in a certain sense the most important result. The proof includes a recognition of the fact that the electric forces can disentangle themselves from material bodies, and can continue to subsist as conditions or changes in the state of space. The details of the experiments further prove that the particular manner in which the electric force is propagated exhibits the closest analogy [1] with the propagation of light; indeed, that it corresponds almost completely to it. The hypothesis that light is an electrical phenomenon is thus made highly probable. To give a strict proof of this hypothesis would logically require experiments upon light itself.

What we here indicate as having been accomplished by the experiments is accomplished independently of the correctness of particular theories. Nevertheless, there is an obvious connection between the experiments and the theory in connection with which they were really undertaken. Since the year 1861 science has been in possession of a theory which Maxwell constructed upon Faraday's views, and which we therefore call the Faraday-Maxwell theory. This theory affirms the possibility of the class of phenomena here discovered just as positively as the remaining electrical theories are compelled to deny it. From the outset Maxwell's theory excelled all others in elegance and in the abundance of the relations between the various phenomena which it included. The probability of this theory, and therefore the number of its adherents, increased from year to year. But as long as Maxwell's theory depended solely upon the probability of its results, and not on the certainty of its hypotheses, it could not completely displace the theories which were opposed to it. The fundamental hypotheses of Maxwell's theory contradicted the usual views,

[1] The analogy does not consist only in the agreement between the more or less accurately measured velocities. The approximately equal velocity is only one element among many others.

and did not rest upon the evidence of decisive experiments. In this connection we can best characterise the object and the result of our experiments by saying: The object of these experiments was to test the fundamental hypotheses of the Faraday-Maxwell theory, and the result of the experiments is to confirm the fundamental hypotheses of the theory.

B. Theoretical

And now, to be more precise, what is it that we call the Faraday-Maxwell theory? Maxwell has left us as the result of his mature thought a great treatise on Electricity and Magnetism; it might therefore be said that Maxwell's theory is the one which is propounded in that work. But such an answer will scarcely be regarded as satisfactory by all scientific men who have considered the question closely. Many a man has thrown himself with zeal into the study of Maxwell's work, and, even when he has not stumbled upon unwonted mathematical difficulties, has nevertheless been compelled to abandon the hope of forming for himself an altogether consistent conception of Maxwell's ideas. I have fared no better myself. Notwithstanding the greatest admiration for Maxwell's mathematical conceptions, I have not always felt quite certain of having grasped the physical significance of his statements. Hence it was not possible for me to be guided in my experiments directly by Maxwell's book. I have rather been guided by Helmholtz's work, as indeed may plainly be seen from the manner in which the experiments are set forth. But unfortunately, in the special limiting case of Helmholtz's theory which leads to Maxwell's equations, and to which the experiments pointed, the physical basis of Helmholtz's theory disappears, as indeed it always does, as soon as action-at-a-distance is disregarded. I therefore endeavoured to form for myself in a consistent manner the necessary physical conceptions, starting from Maxwell's equations, but otherwise simplifying Maxwell's theory as far as possible by eliminating or simply leaving out of consideration those portions which could be dispensed with,

inasmuch as they could not affect any possible phenomena. This explains how the two theoretical papers (forming the conclusion of this collection) came to be written. Thus the representation of the theory in Maxwell's own work, its representation as a limiting case of Helmholtz's theory, and its representation in the present dissertations—however different in form—have substantially the same inner significance. This common significance of the different modes of representation (and others can certainly be found) appears to me to be the undying part of Maxwell's work. This, and not Maxwell's peculiar conceptions or methods, would I designate as "Maxwell's Theory." To the question, "What is Maxwell's theory?" I know of no shorter or more definite answer than the following:—Maxwell's theory is Maxwell's system of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory; every theory which leads to different equations, and therefore to different possible phenomena, is a different theory. Hence in this sense, and in this sense only, may the two theoretical dissertations in the present volume be regarded as representations of Maxwell's theory. In no sense can they claim to be a precise rendering of Maxwell's ideas. On the contrary, it is doubtful whether Maxwell, were he alive, would acknowledge them as representing his own views in all respects.

The very fact that different modes of representation contain what is substantially the same thing, renders the proper understanding of any one of them all the more difficult. Ideas and conceptions which are akin and yet different may be symbolised in the same way in the different modes of representation. Hence for a proper comprehension of any one of these, the first essential is that we should endeavour to understand each representation by itself without introducing into it the ideas which belong to another. Perhaps it may be of service to many of my colleagues if I here briefly explain the fundamental conceptions of the three representations of Maxwell's theory to which I have already referred. I shall thus have an opportunity of stating wherein lies, in my opinion, the especial difficulty of Maxwell's own representation. I can-

not agree with the oft-stated opinion that this difficulty is of a mathematical nature.

When we see bodies acting upon one another at a distance, we can form for ourselves various conceptions of the nature of this action. We may regard the effect as being that of a direct action-at-a-distance, springing across space, or we may regard it as the consequence of an action which is propagated from point to point in a hypothetical medium. Meanwhile, in applying these conceptions to electricity, we can make a series of finer distinctions. As we pass from the pure conception of direct attraction to the pure conception of indirect (*vermittelten*) attraction, we can distinguish between four standpoints.

From the first standpoint we regard the attraction of two bodies as a kind of spiritual affinity between them. The force which each of the two exerts is bound up with the presence of the other body. In order that force should be present at all, there must be at least two bodies present. In some way a magnet only obtains its force when another magnet is brought into its neighbourhood. This conception is the pure conception of action-at-a-distance, the conception of Coulomb's law. In the theory of electricity it has almost been abandoned, but it is still used in the theory of gravitation. Mathematical astronomy speaks of the attraction between the sun and a planet, but with attraction in empty space it has no concern.

From the second standpoint we still regard the attraction of the bodies as a kind of spiritual influence of each upon the other. But although we admit that we can only notice this action when we have at least two bodies, we further assume that each of the acting bodies continually strives to excite at all surrounding points attractions of definite magnitude and direction, even if no other similar bodies happen to be in the neighbourhood. With these strivings, varying always from point to point, we fill (according to this conception) the surrounding space. At the same time we do not assume that there is any change at the place where the action is exerted; the acting body is still both the seat and the source of the force. This is about the standpoint of the potential theory. It obviously

is also the standpoint of certain chapters in Maxwell's work, although it is not the standpoint of Maxwell's theory. In order to compare these conceptions more easily with one another, we represent from this standpoint (as in Fig. 2) two oppositely electrified condenser-plates. The diagrammatic representation will be easily understood; upon the plates are seen the positive and negative electricities (as if they were material); the force between the plates is indicated by arrows. From this standpoint it is immaterial whether the space between the plates is full or empty. If we admit the existence of the light-ether, but suppose that it is removed from a part B of the space, the force will still remain unaltered in this space.

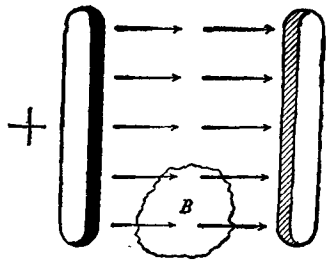


Fig. 2. [II.]

The third standpoint retains the conceptions of the second, but adds to them a further complication. It assumes that the action of the two separate bodies is not determined solely by forces acting directly at a distance. It rather assumes that the forces induce changes in the space (supposed to be nowhere empty), and that these again give rise to new distance-forces (*Fernkräften*). The attractions between the separate bodies depend, then, partly upon their direct action, and partly upon the influence of the changes in the medium. The change in the medium itself is regarded as an electric or magnetic polarisation of its smallest parts under the influence of the acting force. This view has been developed by Poisson with respect to statical phenomena in magnetism, and has been transferred by Mosotti to electrical phenomena. In its most general development, and in its extension over the whole domain of electromagnetism, it is represented by Helmholtz's theory.[1]

Fig. 3 illustrates this standpoint for the case in which the medium plays only a small part in the resultant action. Upon the plates are seen the free electricities, and in the parts

[1] At the end of the paper "On the Equations of Motion of Electricity for Conducting Bodies at Rest."—*Ges. Abh.* 1, p. 545.

of the dielectric the electrical fluids which are separated, but which cannot be divorced from each other. Let us suppose that the space between the plates contains only the light-ether, and let a space, such as B, be hollowed out of this; the forces will then remain in this space, but the polarisation will disappear.

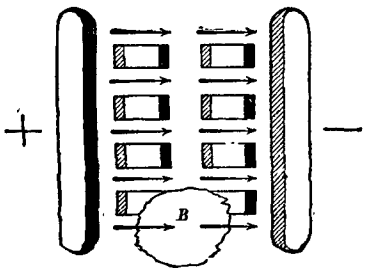


Fig. 3. [111a.]

One limiting case of this mode of conception is of especial importance. As closer examination shows, we can split up the resultant action (which alone can be observed) of material bodies upon one another into an influence due to direct action-at-a-distance, and an influence due to the intervening medium. We can increase that part of the total energy which has its seat in the electrified bodies at the expense of that part which we seek in the medium, and conversely. Now in the limiting case we seek the whole of the energy in the medium. Since no energy corresponds to the electricities which exist upon the conductors, the distance-forces must become infinitely small. But for this it is a necessary condition that no free electricity should be present. The electricity must therefore behave itself like an incompressible fluid. Hence we have only closed currents; and hence arises the possibility of extending the theory to all kinds of electrical disturbances in spite of our ignorance of the laws of unclosed currents.

The mathematical treatment of this limiting case leads us to Maxwell's equations. We therefore call this treatment a form of Maxwell's theory. The limiting case is so called also by v. Helmholtz. But in no sense must this be taken as meaning that the physical ideas on which it is based are Maxwell's ideas.

Fig. 4 indicates the state of the space between two electrified plates in accordance with the conceptions of this theory. The distance-forces have become merely nominal. The electricity on the conductors is still present, and is a necessary part of the conception, but its action-at-a-distance is

completely neutralised by the opposite electricity of the medium which is displaced towards it. The pressure which this medium exerts, on account of the attraction of its internal electrifications, tends to draw the plates together. In the empty space B there are present only vanishingly small distance-forces.

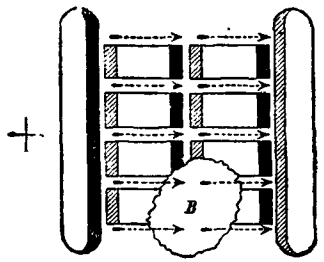


Fig. 4. [IIIb]

The fourth standpoint belongs to the pure conception of action through a medium. From this standpoint we acknowledge that the changes in space assumed from the third standpoint are actually present, and that it is by means of them that material bodies act upon one another. But we do not admit that these polarisations are the result of distance-forces; indeed, we altogether deny the existence of these distance-forces; and we discard the electricities from which these forces are supposed to proceed. We now rather regard the polarisations as the only things which are really present; they are the cause of the movements of ponderable bodies, and of all the phenomena which allow of our perceiving changes in these bodies. The explanation of the nature of the polarisations, of their relations and effects, we defer, or else seek to find out by mechanical hypotheses; but we decline to recognise in the electricities and distance-forces which have hitherto passed current a satisfactory explanation of these relations and effects. The expressions electricity, magnetism, etc., have no further value for us beyond that of abbreviations.

Considered from the mathematical point of view, this fourth mode of treatment may be regarded as coinciding completely with the limiting case of the third. But from the physical point of view the two differ fundamentally. It is impossible to deny the existence of distance-forces, and at the same time to regard them as the cause of the polarisations. Whatever we may designate as "electricity" from this standpoint does not behave like an incompressible fluid. If we consider Fig. 5, which brings symbolically before us the view presented from this standpoint, we are struck by another distinction. The polarisation of the space is represented by the

same symbolic method as was employed in discussing the third standpoint. But whereas in Figs. 3 and 4 this mode of representation explained the nature of the polarisation through the nature of electricity (assumed to be known), we have here to regard the mode of representation as defining the nature of an electric charge through the state of polarisation of the space (regarded as known). Each particle of the dielectric here appears charged in opposite senses with electricity, just as it did from our third standpoint. If we again remove the ether from the space *B*, there remains nothing whatsoever in this space which could remind us of the electrical disturbance in the neighbourhood.'

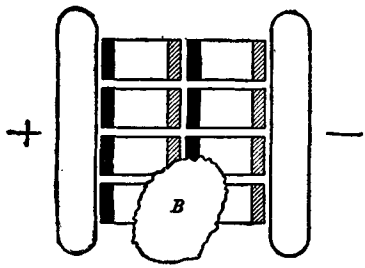


Fig. 5. [IV.]

Now this fourth standpoint, in my opinion, is Maxwell's standpoint. The general explanations in his work leave no room for doubting that he wished to discard distance-forces entirely. He expressly says that if the force or "displacement" in a dielectric is directed towards the right hand, we must conceive each particle of the dielectric as being charged with negative electricity on the right-hand side, and with positive electricity on the left-hand side. But it cannot be denied that other statements made by Maxwell appear at first sight to contradict the conceptions of this standpoint. Maxwell assumes that electricity also exists in conductors; and that this electricity always moves in such a way as to form closed currents with the displacements in the dielectric. The statement that electricity moves like an incompressible fluid is a favourite statement of Maxwell's. But these statements do not fit in with the conceptions of the fourth standpoint; they lead one to suspect that Maxwell rather viewed things from the third point of view. My own opinion is that this was never really the case; that the contradictions are only apparent and arise from a misunderstanding as to words. The following, if I am not mistaken, is the state of affairs:— Maxwell originally developed his theory with the aid of very definite and special conceptions as to the nature of electrical

phenomena. He assumed that the pores of the ether and of all bodies were filled with an attenuated fluid, which, however, could not exert forces at a distance. In conductors this fluid moved freely, and its motion formed what we call an electric current. In insulators this fluid was confined to its place by elastic forces, and its "displacement" was regarded as being identical with electric polarisation. The fluid itself, as being the cause of all electric phenomena, Maxwell called "electricity." Now, when Maxwell composed his great treatise, the accumulated hypotheses of this earlier mode of conception no longer suited him, or else he discovered contradictions in them, and so abandoned them. But he did not eliminate them completely; quite a number of expressions remained which were derived from his earlier ideas. And so, unfortunately, the word "electricity," in Maxwell's work, obviously has a double meaning. In the first place, he uses it (as we also do) to denote a quantity which can be either positive or negative, and which forms the starting-point of distance-forces (or what appear to be such). In the second place, it denotes that hypothetical fluid from which no distance-forces (not even apparent ones) can proceed, and the amount of which in any given space must, under all circumstances, be a positive quantity. If we read Maxwell's explanations and always interpret the meaning of the word "electricity" in a suitable way, nearly all the contradictions which at first are so surprising can be made to disappear. Nevertheless, I must admit that I have not succeeded in doing this completely, or to my entire satisfaction; otherwise, instead of hesitating, I would speak more definitely.[1]

Whether this is so or not, an attempt has been made, in the two theoretical papers here printed, to exhibit Maxwell's theory, *i.e.* Maxwell's system of equations, from this fourth standpoint. I have endeavoured to avoid from the beginning the introduction of any conceptions which are foreign to this standpoint and which might afterwards have to be removed.[2]

[1] M. Poincaré, in his treatise "Electricité et Optique" (vol. i., *Les Théories de Maxwell*), expresses a similar opinion. Herr L. Boltzmann, in his *Vorlesungen über Maxwell's Theorie*, appears like myself to aim at a consistent development of Maxwell's system rather than an exact rendering of Maxwell's thoughts. But no definite opinion can be given, inasmuch as the work is not yet completed.

[2] The expression "electric force" in these papers is only another name for a state of polarisation of space. It would perhaps have been better, in order to

I have further endeavoured in the exposition to limit as far as possible the number of those conceptions which are arbitrarily introduced by us, and only to admit such elements as cannot be removed or altered without at the same time altering possible experimental results. It is true, that in consequence of these endeavours, the theory acquires a very abstract and colourless appearance. It is not particularly pleasing to hear general statements made about "directed changes of state," where we used to have placed before our eyes pictures of electrified atoms. It is not particularly satisfactory to see equations set forth as direct results of observation and experiment, where we used to get long mathematical deductions as apparent proofs of them. Nevertheless, I believe that we cannot, without deceiving ourselves, extract much more from known facts than is asserted in the papers referred to. If we wish to lend more colour to the theory, there is nothing to prevent us from supplementing all this and aiding our powers of imagination by concrete representations of the various conceptions as to the nature of electric polarisation, the electric current, etc. But scientific accuracy requires of us that we should in no wise confuse the simple and homely figure, as it is presented to us by nature, with the gay garment which we use to clothe it. Of our own free will we can make no change whatever in the form of the one, but the cut and colour of the other we can choose as we please.

Such further remarks as I may wish to make on points of detail will be found at the end of the book as supplementary notes.

prevent misconceptions, if I had replaced it by another expression, such, for example, as "electric field-intensity," or "*elektrische Intensität*," which Herr E. Cohn proposes in his paper, which refers to the same subject ("Zur Systematik der Elektricitätslehre," *Wied. Ann.* **40**, p. 625, 1890).

II

ON VERY RAPID ELECTRIC OSCILLATIONS

(*Wiedemann's Annalen*, **31**, p. 421, 1887.)

THE electric oscillations of open induction-coils have a period of vibration which is measured by ten-thousandths of a second. The vibrations in the oscillatory discharges of Leyden jars, such as were observed by Feddersen,[1] follow each other about a hundred times as rapidly. Theory admits the possibility of oscillations even more rapid than these in open wire circuits of good conductivity, provided that the ends are not loaded with large capacities; but at the same time theory does not enable us to decide whether such oscillations can be actually excited on such a scale as to admit of their being observed. Certain phenomena led me to expect that oscillations of the latter kind do really occur under certain conditions, and that they are of such strength as to allow of their effects being observed. Further experiments confirmed my expectation, and I propose to give here an account of the experiments made and the phenomena observed.

The oscillations which are here dealt with are about a hundred times as rapid as those observed by Feddersen. Their period of oscillation—estimated, it is true, only by the aid of theory—is of the order of a hundred-millionth of a second. Hence, according to their period, these oscillations range themselves in a position intermediate between the acoustic oscillations of ponderable bodies and the light-oscillations of the ether. In this, and in the possibility that a closer observa-

[1] For the literature see Colley, *Wied. Ann.* **26**, p. 432, 1885. (See also Note 1 at the end of this book.)

tion of them may be of service in the theory of electrodynamics, lies the interest which they present.

Preliminary Experiments

If, in addition to the ordinary spark-gap of an induction-coil, there be introduced in its discharging circuit a Riess's spark-micrometer, the poles of which are joined by a long metallic shunt, the discharge follows the path across the air-gap of the micrometer in preference to the path along the metallic conductor, so long as the length of the air-gap does not exceed a certain limit. This is already known, and the construction of lightning-protectors for telegraph-lines is based on this experimental fact. It might be expected that, if the metallic shunt were only made short and of low resistance, the sparks in the micrometer would then disappear. As a matter of fact, the length of the sparks obtained does diminish with the length of the shunt, but the sparks can scarcely be made to disappear entirely under any circumstances. Even when the two knobs of the micrometer are connected by a few centimetres of thick copper wire, sparks can still be observed, although they are exceedingly short. This experiment shows directly that at the instant when the discharge occurs the potential along the circuit must vary in value by hundreds of volts even in a few centimetres; indirectly it proves with what extraordinary rapidity the discharge takes place. For the difference of potential between the knobs of the micrometer can only be regarded as an effect of self-induction in the metallic shunt. The time in which the potential of one of the knobs is appreciably changed is of the same order as the time in which such a change is transmitted to the other knob through a short length of a good conductor. The potential difference between the micrometer-knobs might indeed be supposed to be determined by the resistance of the shunt, the current-density during the discharge being possibly large. But a closer examination of the quantitative relations shows that this supposition is inadmissible; and the following experiment shows independently that this conjecture cannot be put forward. We again connect the knobs of the micrometer by a good metallic conductor, say by a copper wire 2 mm. in

diameter and 0·5 metre long, bent into rectangular form; we do not, however, introduce this into the discharging-circuit of the induction-coil, but we simply place one pole of it in communication with any point of the discharging circuit by means of a connecting wire. (Fig. 6 shows the arrangement of the apparatus; *A* represents diagrammatically the induction-coil, *B* the discharger, and *M* the micrometer.) Thereupon we again observe, while the induction-coil is working, a stream of sparks in the micrometer which may, under suitable conditions, attain a length of several millimetres. Now this experiment shows, in the first place, that at the instant when the discharge takes place violent electrical disturbances occur, not only in the actual discharging-circuit, but also in all conductors connected with it. But, in the second place, it shows more clearly than the preceding experiment that these disturbances run on so rapidly that even the time taken by electrical waves in rushing through short metallic conductors becomes of appreciable importance. For the experiment can only be interpreted in the sense that the change of potential proceeding from the induction-coil reaches the knob 1 in an appreciably shorter time than the knob 2. The phenomenon may well cause surprise when we consider that, as far as we know, electric waves in copper wires are propagated with a velocity which is approximately the same as that of light. So it appeared to me to be worth while to endeavour to determine what conditions were most favourable for the production of brilliant sparks in the micrometer. For the sake of brevity we shall speak of these sparks as the side-sparks (in order to distinguish them from the discharge proper), and of the micrometer discharging-circuit as the side-circuit (*Nebenkreis*).

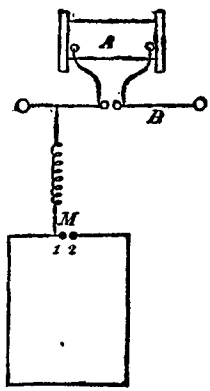


Fig. 6.

First of all it became evident that powerful discharges are necessary if side-sparks of several millimetres in length are desired. I therefore used in all the following experiments a large Ruhmkorff coil, 52 cm. long and 20 cm. in diameter, which was provided with a mercury interrupter and was excited by six large Bunsen cells. Smaller induction-coils

gave the same qualitative results, but the side-sparks were shorter, and it was therefore more difficult to observe differences between them. The same held good when discharges from Leyden jars or from batteries were used instead of the induction-coil. It further appeared that even when the same apparatus was used a good deal depended upon the nature of the exciting spark in the discharger (B). If this takes place between two points, or between a point and a plate, it only gives rise to very weak side-sparks; discharges in rarefied gases or through Geissler tubes were found to be equally ineffective. The only kind of spark that proved satisfactory was that between two knobs (spheres), and this must neither be too long nor too short. If it is shorter than half a centimetre the side-sparks are weak, and if it is longer than $1\frac{1}{2}$ cm. they disappear entirely.

In the following experiments I used, as being the most suitable, sparks three-quarters of a centimetre long between two brass knobs of 3 cm. diameter. Even these sparks were not always equally efficient; the most insignificant details, often without any apparent connection, resulted in useless sparks appearing instead of active ones. After some practice one can judge from the appearance and noise of the sparks whether they are such as are able to excite side-sparks. The active sparks are brilliant white, slightly jagged, and are accompanied by a sharp crackling. That the spark in the discharger is an essential condition of the production of shunt-sparks is easily shown by drawing the discharger-knobs so far apart that the distance between them exceeds the sparking distance of the induction-coil; every trace of the side-sparks then disappears, although the differences of potential now present are greater than before.

The length of the micrometer-circuit naturally has great influence upon the length of the sparks in it. For the greater this distance, the greater is the retardation which the electric wave suffers between the time of its arrival at the one knob and at the other. If the side-circuit is made very small, the side-sparks become extremely short; but it is scarcely possible to prepare a circuit in which sparks will not show themselves under favourable circumstances. Thus, if you file the ends of a stout copper wire, 4-6 cm. long, to sharp points,

bend it into an almost closed circuit, insulate it and now touch the discharger with this small wire circuit, a stream of very small sparks between the pointed ends generally accompanies the discharges of the induction-coil. The thickness and material (and therefore the resistance) of the side-circuit have very little effect on the length of the side-sparks. We were therefore justified in declining to attribute to the resistance the differences of potential which arise. And according to our conception of the phenomenon, the fact that the resistance is of scarcely any importance can cause us no surprise; for, to a first approximation, the rate of propagation of an electric wave along a wire depends solely upon its capacity and self-induction, and not upon its resistance. The length of the wire which connects the side-circuit to the principal circuit has also little effect, provided it does not exceed a few metres. We must assume that the electric disturbance which proceeds from the principal circuit travels along it without suffering any real change of intensity.

On the other hand, the position of the point at which contact with the side-circuit is made has a very noteworthy effect upon the length of the sparks in it. We should expect this to be so if our interpretation of the phenomenon is correct. For if the point of contact is so placed that the paths from it to the two knobs of the micrometer are of equal length, then every variation which passes through the connecting wire will arrive at the two knobs in the same phase, so that no difference of potential between them can arise. Experiment confirms this supposition. Thus, if we shift the point of contact on the side-circuit, which we have hitherto supposed near one of the micrometer-knobs, farther and farther away from this, the spark-length diminishes, and in a certain position the sparks disappear completely or very nearly so; they become stronger again in proportion as the contact approaches the second micrometer-knob, and in this position attain the same length as in the first. The point at which the spark-length is a minimum may be called the null-point. It can generally be determined to within a few centimetres. It always divides the length of the wire between the two micrometer-knobs into very nearly equal parts. If the conductor is symmetrical on the right and left of the line joining the micrometer and the

null-point, the sparks always disappear completely, the phenomenon can be observed even with quite short side-circuits. Fig. 7 shows a convenient arrangement of the experiment; *a b c d* is a rectangle of bare copper wire 2 mm. in diameter, insulated upon sealing-wax supports;[1] in my experiments it was 80 cm. broad and 125 long. When the connecting wire is attached to either of the knobs 1 and 2, or either of the points *a* and *b*, sparks 3-4 mm. long pass between 1 and 2; no sparks can be obtained when the connection is at the point *e*, as in the figure; shifting the contact a few centimetres to right or left causes the sparks to reappear. It should be remarked that we consider sparks as being perceptible when they are only a few hundredths of a millimetre in length.

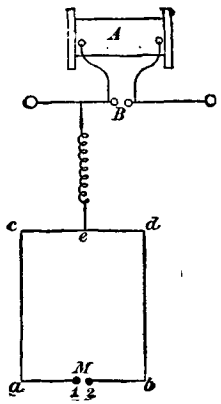


Fig. 7.

The following experiment shows that the above is not a complete representation of the way in which things go on. For if, after the contact has been adjusted so as to make the sparks disappear, we attach to one of the micrometer-knobs another conductor projecting beyond it, active sparking again occurs. This conductor, being beyond the knob, cannot affect the simultaneous arrival of the waves travelling from *e* to 1 and 2. But it is easy to see what the explanation of this experiment is. The waves do not come to an end after rushing once towards *a* and *b*; they are reflected and traverse the side-circuit several, perhaps many, times and so give rise to stationary oscillations in it. If the paths *e c a* 1 and *e d b* 2 are equal, the reflected waves will again arrive at 1 and 2 simultaneously. If, however, the wave reflected from one of the knobs is missing, as in the last experiment, then, although the first disturbance proceeding from *e* will not give rise to sparks, the reflected waves will. We must therefore imagine the abrupt variation which arrives at *e* as creating in the side-circuit the oscillations which are natural to it, much as the blow of a hammer produces in an elastic rod its natural vibrations. If this idea is correct, then

[1] [See Note 2 at end of book.]

the condition for disappearance of sparks in *M* must substantially be equality of the vibration-periods of the two portions *e* 1 and *e* 2. These vibration-periods are determined by the product of the coefficient of self-induction of those parts of the conductor into the capacity of their ends; they are practically independent of the resistance of the branches. The following experiments may be applied to test these considerations and are found to agree with them:—

If the connection is placed at the null-point and one of the micrometer-knobs is touched with an insulated conductor, sparking begins again because the capacity of the branch is increased. An insulated sphere of 2-4 cm. diameter is quite sufficient. The larger the capacity which is thus added, the more energetic becomes the sparking. Touching at the null-point has no influence since it affects both branches equally. The effect of adding a capacity to one branch is annulled by adding an equal capacity to the other. It can also be compensated by shifting the connecting wire in the direction of the loaded branch, *i.e.* by diminishing the self-induction of the latter. The addition of a capacity produces the same effect as increasing the coefficient of self-induction. If one of the branches be cut and a few centimetres or decimetres of coiled copper wire introduced into it, sparking begins again. The change thus produced can be compensated by inserting an equal length of copper wire in the other branch, or by shifting the copper wire towards the branch which was altered, or by adding a suitable capacity to the other branch. Nevertheless, it must be remarked that when the two branches are not of like kind, a complete disappearance of the sparks cannot generally be secured, but only a minimum of the sparking distance.

The results are but little affected by the resistance of the branch. If the thick copper wire in one of the branches was replaced by a much thinner copper wire or by a wire of German silver, the equilibrium was not disturbed, although the resistance of the one branch was a hundred times that of the other. Very large fluid resistances certainly made it impossible to secure a disappearance of the sparks, and short air-spaces introduced into one of the branches had a like effect.

The self-induction of iron wires for slowly alternating currents is about eight to ten times as great as that of copper

wires of equal length and thickness. I therefore expected that short iron wires would produce equilibrium with longer copper wires. This expectation was not confirmed; the branches remained in equilibrium when a copper wire was replaced by an iron wire of equal length. If the theory of the observations here given is correct, this can only mean that the magnetism of iron is quite unable to follow oscillations so rapid as those with which we are here concerned, and that it, therefore, is without effect. A further experiment which will be described below appears to point in the same direction.

Induction-Effects of unclosed Currents

The sparks which occur in the preceding experiments owe their origin, according to our supposition, to self-induction, but if we consider that the induction-effects in question are derived from exceedingly weak currents in short, straight conductors, there appears to be good reason to doubt whether these do really account satisfactorily for the sparks. In order to settle this doubt I tried whether the observed electrical disturbances did not manifest effects of corresponding magnitude in neighbouring conductors. I therefore bent some copper wire into the form of rectangular circuits, about 10-20 cm. in the side, and containing only very short spark-gaps. These were insulated and brought near to the conductors in which the disturbances took place, and in such a position that a side of the rectangle was parallel to the conductor. When the rectangle was brought sufficiently near, a stream of sparks in it always accompanied the discharges of the induction-coil. These sparks were most brilliant in the neighbourhood of the discharger, but they could also be observed along the wire leading to the side-circuit as well as in the branches of the latter. The absence of any direct discharge between the inducing and induced circuits was carefully verified, and was also prevented by the introduction of a solid insulator. Thus it is scarcely possible that our conception of the phenomenon is erroneous. That the induction between two simple straight lengths of wire, traversed by only small quantities of electricity, can yet become strong enough to produce sparks, shows again the extraordinary shortness of

the time in which these small quantities of electricity must pass backwards and forwards along the conductors.

In order to study the phenomena more closely, the rectangle which at first was employed as the side-circuit was again brought into use, but this time as the induced circuit. Along the short side of this (as indicated in Fig. 8) and at a distance of 3 cm. from it was stretched a second copper wire *g h*, which was placed in connection with any part of the discharger. As long as the end *h* of the wire *g h* was free, only weak sparks appeared in the micrometer *M*, and these were due to the discharge-current of the wire *g h*. But if an insulated conductor *C*—one taken from an electrical machine—was then attached to *h*, so that larger quantities of electricity had to pass through the wire, sparks up to two millimetres long appeared in the micrometer. This was not caused by an electrostatic effect of the conductor, for if it was attached to *g* instead of to *h*, it was without effect; and the action was not due to the charging-current of the conductor, but to the sudden discharge brought about by the sparks. For when the knobs of the discharger were drawn so far apart that sparks could no longer spring across it, then the sparks disappeared completely from the induced circuit as well. Not every kind of spark produced a sufficiently energetic discharge; here, again, only such sparks as were before found to occasion powerful side-sparks were found to be effective in exciting the inductive action. The sparks excited in the secondary circuit passed not only between the knobs of the micrometer but also from these to other insulated conductors held near. The length of the sparks was notably diminished by attaching to the knobs conductors of somewhat large capacity or touch-

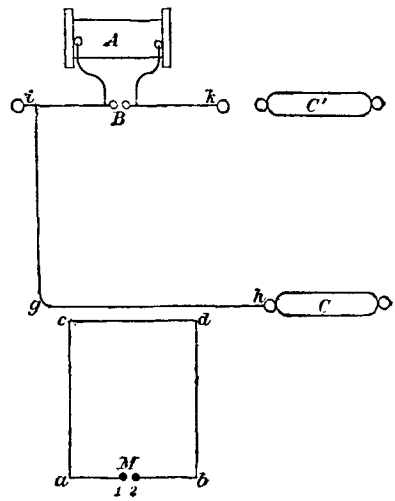


Fig. 8.

ing one of them with the hand; clearly the quantities of electricity set in motion were too small to charge conductors of rather large capacity to the full potential. On the other hand, the sparking was not much affected by connecting the two micrometer-knobs by a short wet thread. No physiological effects of the induced current could be detected; the secondary circuit could be touched or completed through the body without experiencing any shock.

Certain accessory phenomena induced me to suspect that the reason why the electric disturbance in the wire $g\,h$ produced such a powerful inductive action lay in the fact that it did not consist of a simple charging-current, but was rather of an oscillatory nature. I therefore endeavoured to strengthen the induction by modifying the conditions so as to make them more favourable for the production of powerful oscillations. The following arrangement of the experiment suited my purpose particularly well. I attached the conductor C as before to the wire $g\,h$ and then separated the micrometer-knobs so far from each other that sparks only passed singly. I then attached to the free pole of the discharger k (Fig. 8) a second conductor C' of about the same size as the first. The sparking then again became very active, and on drawing the micrometer-knobs still farther apart decidedly longer sparks than at first could be obtained. This cannot be due to any direct action of the portion of the circuit $i\,k$, for this would diminish the effect of the portion $g\,h$; it must, therefore, be due to the action of the conductor C' upon the discharge-current of C. Such an action would be incomprehensible if we assumed that the discharge of the conductor C was aperiodic. It becomes, however, intelligible if we assume that the inducing current in $g\,h$ consists of an electric oscillation which, in the one case, takes place in the circuit C—wire $g\,h$—discharger, and in the other in the system C—wire $g\,h$, wire $i\,k$—C'. It is clear in the first place that the natural oscillations of the latter system would be the more powerful, and in the second place that the position of the spark in it is more suitable for exciting the vibration.

Further confirmation of these views may be deferred for the present. But here we may bring forward in support of them the fact that they enable us to give a more correct explanation

of the part which the discharge of the Ruhmkorff coil plays in the experiment. For if oscillatory disturbances in the circuit C—C' are necessary for the production of powerful induction-effects, it is not sufficient that the spark in this circuit should be established in an exceedingly short time, but it must also reduce the resistance of the circuit below a certain value, and in order that this may be the case the current-density from the very start must not fall below a certain limit. Hence it is that the inductive effect is exceedingly feeble when the conductors C and C' are charged by means of an electrical machine[1] (instead of a Ruhmkorff coil) and then allowed to discharge themselves; and that it is also very feeble when a small coil is used, or when too large a spark-gap is introduced; in all these cases the motion is aperiodic. On the other hand, a powerful discharge from a Ruhmkorff coil gives rise to oscillations, and therefore to powerful disturbances all round, by performing the following functions:—In the first place, it charges the ends C and C' of the system to a high potential; secondly, it gives rise to a disruptive discharge; and thirdly, after starting the discharge, it keeps the resistance of the air-gap so low that oscillations can take place. It is known that if the capacity of the ends of the system is large—if, for example, they consist of the armatures of a battery of Leyden jars—the discharge-current from these capacities is able of itself to reduce the resistance of the spark-gap considerably; but when the capacities are small this function must be performed by some extraneous discharge, and for this reason the discharge of the induction-coil is, under the conditions of our experiment, absolutely necessary for exciting oscillations.

As the induced sparks in the last experiment were several millimetres long, I had no doubt that it would be possible to obtain sparks even when the wires used were much farther apart; I therefore tried to arrange a modification of the experiment which appeared interesting. I gave the inducing circuit the form of a straight line (Fig. 9). Its ends were formed by the conductors C and C'. These were 3 metres apart, and were connected by a copper wire 2 mm. thick, at the centre of which was the discharger of the induction-coil. The induced circuit was the same as in the preceding experi-

[1] [See Note 3 at end of book.]

ment, 120 cm. long and 80 cm. broad. If the shortest distance between the two systems was now made equal to 50 cm., induced sparks 2 mm. in length could still be obtained; at greater distances the spark-length decreased

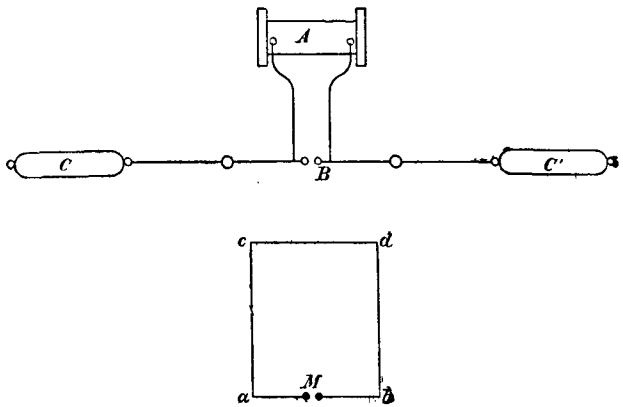


Fig. 9.

rapidly, but even when the shortest distance was 1·5 metres, a continuous stream of sparks was perceptible. The experiment was in no way interfered with if the observer moved between the inducing and induced systems. A few control-experiments again established the fact that the phenomena observed were really caused by the current in the rectilinear portion. If one or both halves of this were removed, the sparks in the micrometer ceased, even when the coil was still in action. They also ceased when the knobs of the discharger were drawn so far apart as to prevent any sparking in it. Inasmuch as the difference of electrostatic potential at the ends of the conductors C and C' are now greater than before, this shows that these differences of potential are not the cause of the sparks in the micrometer.

Hitherto the induced circuit was closed; it was, however, to be supposed that the induction would take place equally in an open circuit. A second insulated copper wire was therefore stretched parallel to the straight wire in the preceding arrangement, and at a distance of 60 cm. from it. This second wire was shorter than the first; two insulated spheres 10 cm. in diameter were attached to its ends and the spark-micrometer was introduced in the middle of it. When the coil was now

started, the stream of sparks from it was accompanied by a similar stream in the secondary conductor. But this experiment should be interpreted with caution, for the sparks observed are not solely due to electromagnetic induction. The alternating motion in the system C C' is indeed superposed upon the Ruhmkorff discharge itself. But during its whole course the latter determines an electrification of the conductor C, and an opposite electrification of the conductor C'. These electrifications had no effect upon the closed circuit in the preceding experiment, but in the present discontinuous conductor they induce by purely electrostatic action opposite electrifications in the two parts of the conductor, and thus produce sparks in the micrometer. In fact, if we draw the knobs of the discharger so far apart that the sparks in it disappear, the sparks in the micrometer, although weakened, still remain. These sparks represent the effect of electrostatic induction, and conceal the effect which alone we desired to exhibit.

There is, however, an easy way of getting rid of these disturbing sparks. They die away when we interpose a bad conductor between the knobs of the micrometer, which is most simply done by means of a wet thread. The conductivity of this is obviously good enough to allow the current to follow the relatively slow alternations of the discharge from the coil; but in the case of the exceedingly rapid oscillations of the rectilinear circuit it is, as we have already seen, not good enough to bring about an equalisation of the electrifications. If after placing the thread in position we again start the sparking in the primary circuit, vigorous sparking begins again in the secondary circuit, and is now solely due to the rapid oscillations in the primary circuit. I have tested to what distance this action extended. Up to a distance of 1·2 metres between the parallel wires the sparks were easily perceptible; the greatest perpendicular distance at which regular sparking could be observed was 3 metres. Since the electrostatic effect diminishes more rapidly with increasing distance than the electromagnetic induction, it was not necessary to complicate the experiment by using the wet thread at greater distances, for, even without this, only those discharges which excited oscillations in the primary wire were attended by sparks in the secondary circuit.

I believe that the mutual action of rectilinear open circuits which plays such an important part in theory is, as a matter of fact, illustrated here for the first time.

Resonance Phenomena

We may now regard it as having been experimentally proved that currents of rapidly varying intensity, capable of producing powerful induction-effects, are present in conductors which are connected with the discharge circuit. The existence of regular oscillations, however, was only assumed for the purpose of explaining a comparatively small number of phenomena, which might perhaps be accounted for otherwise. But it seemed to me that the existence of such oscillations might be proved by showing, if possible, symphonic relations between the mutually reacting circuits. According to the principle of resonance, a regularly alternating current must (other things being similar) act with much stronger inductive effect upon a circuit having the same period of oscillation than upon one of only slightly different period.[1] If, therefore, we allow two circuits, which may be assumed to have approximately the same period of vibration, to react on one another, and if we vary continuously the capacity or coefficient of self-induction of one of them, the resonance should show that for certain values of these quantities the induction is perceptibly stronger than for neighbouring values on either side.

The following experiments were devised in accordance with this principle, and, after a few trials, they quite answered my intention. The experimental arrangement was very nearly the same as that of Fig. 9, excepting that the circuits were made somewhat different in size. The primary conductor was a perfectly straight copper wire 2·6 metres long and 5 mm. thick. This was divided in the middle so as to include the spark-gap. The two small knobs between which the discharge took place were mounted directly on the wire and connected with the poles of the induction-coil. To the ends of the wire were attached two spheres, 30 cm. in diameter, made of strong zinc-plate. These could be shifted along the wire. As they formed (electrically) the ends of the conductor, the circuit

[1] Cf. Oberbeck, *Wied. Ann.* **26**, p. 245, 1885.

could easily be shortened or lengthened. The secondary circuit was proportioned so that it was expected to have a somewhat smaller period of oscillation than the primary; it was in the form of a square 75 cm. in the side, and was made of copper wire 2 mm. in diameter. The shortest distance between the two systems was made equal to 30 cm., and at first the primary current was allowed to remain of full length. Under these circumstances the length of the biggest spark in the induced circuit was 0·9 mm. When two insulated metal spheres of 8 cm. diameter were placed in contact with the two poles of the circuit, the spark-length increased, and could be made as large as 2·5 mm. by suitably diminishing the distance between the two spheres. On the other hand, if two conductors of very large surface were placed in contact with the two poles, the spark-length was reduced to a small fraction of a millimetre. Exactly similar results followed when the poles of the secondary circuit were connected with the plates of a Kohlrausch condenser. When the plates were far apart the spark-length was increased by increasing the capacity, but when they were brought closer together the spark-length again fell to a very small value. The easiest way of adjusting the capacity of the secondary circuit was by hanging over its two ends two parallel bits of wire and altering the length of these and their distance apart. By careful adjustment the sparking distance was increased to 3 mm., after which it diminished, not only when the wires were lengthened, but also when they were shortened. That an increase of the capacity should diminish the spark-length appeared only natural; but that it should have the effect of increasing it can scarcely be explained excepting by the principle of resonance.

If our interpretation of the above experiment is correct, the secondary circuit, before its capacity was increased, had a somewhat shorter period than the primary. Resonance should therefore have occurred when the rapidity of the primary oscillations was increased. And, in fact, when I reduced the length of the primary circuit in the manner above indicated, the sparking distance increased, again reached a maximum of 3 mm. when the centres of the terminal spheres were 1·5 metres apart, and again diminished when the spheres were brought still closer together. It might be supposed that the

spark-length would now increase still further if the capacity of the secondary circuit were again, as before, increased. But this is not the case; on attaching the same wires, which before had the effect of increasing the spark-length, this latter falls to about 1 mm. This is in accordance with our conception of the phenomenon; that which at first brought about an equality between the periods of oscillation now upsets an equality which has been attained in another way. The experiment was most convincing when carried out as follows:—The spark-micrometer was adjusted for a fixed sparking distance of 2 mm. If the secondary circuit was in its original condition, and the primary circuit 1·5 metres long, sparks passed regularly. If a small capacity is added to the secondary circuit in the way already described, the sparks are completely extinguished; if the primary circuit is now lengthened to 2·6 metres they reappear; they are extinguished a second time if the capacity added to the secondary circuit is doubled; and by continuously increasing the capacity of the already lengthened primary circuit they can be made to appear and disappear again and again. The experiment shows us quite plainly that effective action is determined, not by the condition of either of the circuits, but by a proper relation (or harmony) between the two.

The length of the induced sparks increased considerably beyond the values given above when the two circuits were brought closer together. When the two circuits were at a distance of 7 cm. from one another and were adjusted to exact resonance, it was possible to obtain induced sparks 7 mm. long; in this case the electromotive forces induced in the secondary circuit were almost as great as those in the primary.

In the above experiments resonance was secured by altering the coefficient of self-induction and the capacity of the primary circuit, as well as the capacity of the secondary circuit. The following experiments show that an alteration of the coefficient of self-induction of the secondary circuit can also be used for this purpose. A series of rectangles *a b c d* (Fig. 9) were prepared in which the sides *a b* and *c d* were kept of the same length, but the sides *a c* and *b d* were made of wires varying in length from 10 cm. to 250 cm. A marked maximum of the sparking distance was apparent when the length of the rectangle was 1·8 metres. In order to get an idea of

the quantitative relations I measured the longest sparks which appeared with various lengths of the secondary circuit. Fig. 10*a* shows the results.[1] Abscissæ represent the total length of the induced circuit and ordinates the maximum spark-length. The points indicate the observed values. Measurements of sparking distances are always very uncertain, but this uncertainty cannot be such as to vitiate the general nature of the result. In another set of experiments not only the lengths of the sides *a b* and *c d*, but also their distance apart (30 cm.), and their position were kept constant; but the sides *a c* and *b d* were formed of wires of gradually increasing length coiled into loose spirals. Fig. 10*b* shows the results obtained. The maximum here corresponds with a somewhat greater length of wire than before. Probably this is because the lengthening of the wire in this case increases only the coefficient of self-induction, whereas in the former case it increased the capacity as well.

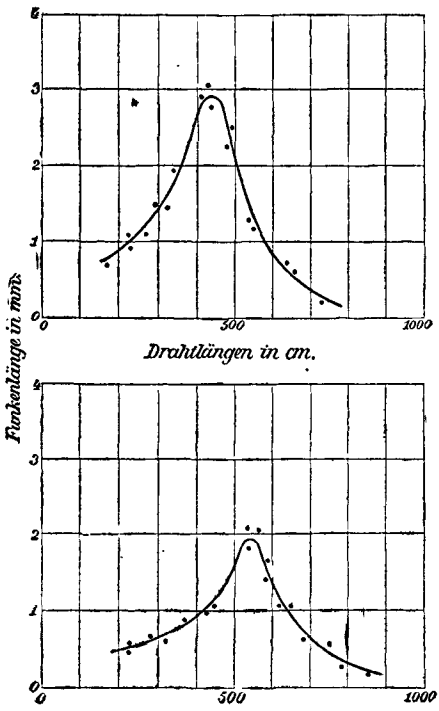


Fig. 10, *a* and *b*.

Some further experiments were made in order to determine whether any different result would be obtained by altering the resistance of the secondary circuit. With this intention the wire *c d* of the rectangle was replaced by various thin copper and German silver wires, so that the resistance of the secondary circuit was made about a hundred times as large. This change had very little effect on the sparking distance, and none at

[1] [See Note 4 at end of book.]

all on the resonance; or, in other words, on the period of oscillation.

The effect of the presence of iron was also examined. The wire *c d* was in some experiments surrounded by an iron tube, in others replaced by an iron wire. Neither of these changes produced a perceptible effect in any sense. This again confirms the supposition that the magnetism of iron cannot follow such exceedingly rapid oscillations, and that its behaviour towards them is neutral. Unfortunately we possess no experimental knowledge as to how the oscillatory discharge of Leyden jars is affected by the presence of iron.

Nodes

The oscillations which we excited in the secondary circuit, and which were measured by the sparks in the micrometer, are not the only ones, but are the simplest possible in that circuit. While the potential at the ends oscillates backwards and forwards continually between two limits, it always retains the same mean value in the middle of the circuit. This middle point is therefore a node of the electric oscillation, and the oscillation has only this one node. Its existence can also be shown experimentally, and that in two ways. In the first place, it can be done by bringing a small insulated sphere near the wire. The mean value of the potential of the small sphere cannot differ appreciably from that of the neighbouring bit of wire. Sparking between the knob and the wire can therefore only arise through the potential of the neighbouring point of the system experiencing sufficiently large oscillations about the mean value. Hence there should be vigorous sparking at the ends of the system and none at all near the node. And this in fact is so, excepting, indeed, that when the nodal point is touched the sparks do not entirely disappear, but are only reduced to a minimum. A second way of showing the nodal point is clearer. Adjust the secondary circuit for resonance and draw the knobs of the micrometer so far apart that sparks can only pass by the assistance of the action of resonance. If any point of the system is now touched with a conductor of some capacity, we should in general expect that the resonance would be

disturbed, and that the sparks would disappear; only at the node would there be no interference with the period of oscillation. Experiment confirms this. The middle of the wire can be touched with an insulated sphere, or with the hand, or can even be placed in metallic connection with the gas-pipes without affecting the sparks; similar interference at the side-branches or the poles causes the sparks to disappear.

After the possibility of fixing a nodal point was thus proved, it appeared to me to be worth while experimenting on the production of a vibration with two nodes. I proceeded as follows:—The straight primary conductor $C\,C'$ and the rectilinear secondary $a\,b\,c\,d$ were set up as in the earlier experiments and brought to resonance. An exactly similar rectangle $e\,f\,g\,h$ was then placed opposite to $a\,b\,c\,d$ as shown in Fig. 11, and the neighbouring poles of both were joined (1 with 3 and 2 with 4). The whole system forms a closed metallic circuit, and the lowest or fundamental vibration possible in it has two nodes. Since the period of this vibration must very nearly agree with the period of either half, and therefore with the period of the primary conductor, it was supposed that vibrations would develop having two antinodes at the junctions 1-3 and 2-4, and two nodes at the middle points of $c\,d$ and $g\,h$. These vibrations were always measured by the sparking distance between the knobs of the micrometer which formed the poles 1 and 2. The results of the experiment were as follows:—Contrary to what was expected, it was found that the sparking distance between 1 and 2 was considerably diminished by the addition of the rectangle $e\,f\,g\,h$. From about 3 mm. it fell to 1 mm. Never-

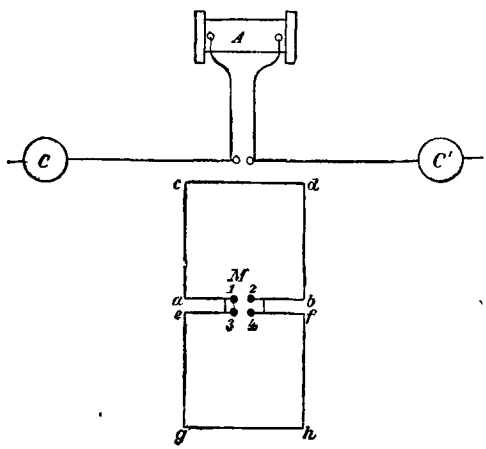


Fig. 11.

theless there was still resonance between the primary circuit and the secondary. For every alteration of *e f g h* reduced the sparking distance still further, and this whether the alteration was in the direction of lengthening or shortening the rectangle. Further, it was found that the two nodes which were expected were actually present. By holding a sphere near *c d* and *g h* only very weak sparks could be obtained as compared with those from *a e* and *b f*. And it could also be shown that these nodes belonged to the same vibration which, when strengthened by resonance, produced the sparks 1-2. For the sparking distance between 1 and 2 was not diminished by touching along *c d* or *g h*, but it was by touching at every other place.

The experiment may be modified by breaking one of the connections 1-3 or 2-4, say the latter. As the current-strength of the induced oscillation is always zero at these points, this cannot interfere much with the oscillation. And, in fact, after the connection has been broken, it can be shown as before that resonance takes place, and that the vibrations corresponding to this resonance have two nodes at the same places. Of course there was this difference, that the vibration with two nodes was no longer the deepest possible vibration; the vibration of longest period would be one with a single node between *a* and *e*, and having the highest potentials at the poles 2 and 4. And if we bring the knobs at these poles nearer together we find that there is feeble sparking between them. We may attribute these sparks to an excitation, even if only feeble, of the fundamental vibration; and this supposition is made almost a certainty by the following extension of the experiment:—We stop the sparks between 1 and 2 and direct our attention to the length of the sparks between 2 and 4, which measures the intensity of the fundamental vibration. We now increase the period of oscillation of the primary circuit by extending it to the full length and adding to its capacity. We observe that the sparks thus increase to a maximum length of several millimetres and then again become shorter. Clearly they are longest when the oscillation of the primary current agrees with the fundamental oscillation. And while the sparks between 2 and 4 are longest it can be easily shown that at this time only a single nodal point corresponds to these sparks. For only between *a* and *e* can the conductor

be touched without interfering with the sparks, whereas touching the previous nodal points interrupts the stream of sparks. Hence it is in this way possible, in any given conductor, to make either the fundamental vibration or the first overtone preponderate.

Meanwhile, there are several further problems which I have not solved; amongst others, whether it is possible to establish the existence of oscillations with several nodes. The results already described were only obtained by careful attention to insignificant details; and so it appeared probable that the answers to further questions would turn out to be more or less ambiguous. The difficulties which present themselves arise partly from the nature of the methods of observation, and partly from the nature of the electric disturbances observed. Although these latter manifest themselves as undoubted oscillations, they do not exhibit the characteristics of perfectly regular oscillations. Their intensity varies considerably from one discharge to another, and from the comparative unimportance of the resonance-effects we conclude that the damping must be rapid; many secondary phenomena point to the superposition of irregular disturbances upon the regular oscillations, as, indeed, was to be expected from the complex nature of the system of conductors. If we wish to compare, in respect of their mathematical relations, our oscillations with any particular kind of acoustic oscillations, we must not choose the long-continued harmonic oscillations of uniform strength which are characteristic of tuning-forks and strings, but rather such as are produced by striking a wooden rod with a hammer, —oscillations which rapidly die away, and with which are mingled irregular disturbances.[1] And when we are dealing with oscillations of the latter class we are obliged, even in acoustics, to content ourselves with mere indications of resonance, formation of nodes, and similar phenomena.

For the sake of those who may wish to repeat the experiments and obtain the same results I must add one remark, the exact significance of which may not be clear at first. In all the experiments described the apparatus was set up in such a way that the spark of the induction-coil was visible from the place where the spark in the micrometer took place. When

[1] [See Note 5 at end of book.]

this is not the case the phenomena are qualitatively the same, but the spark-lengths appear to be diminished. I have undertaken a special investigation of this phenomenon, and intend to publish the results in a separate paper.[1]

Theoretical

It is highly desirable that quantitative data respecting the oscillations should be obtained by experiment. But as there is at present no obvious way of doing this, we are obliged to have recourse to theory, in order to obtain at any rate some indication of the data. The theory of electric oscillations which has been developed by Sir W. Thomson, v. Helmholtz, and Kirchhoff has been verified as far as the oscillations of open induction-coils and oscillatory Leyden jar discharges are concerned;[2] we may therefore feel certain that the application of this theory to the present phenomena will give results which are correct, at least as far as the order of magnitude is concerned.

To begin with, the period of oscillation is the most important element. As an example to which calculation can be applied, let us determine the (simple or half) period of oscillation T of the primary conductor which we used in the resonance-experiments. Let P denote the coefficient of self-induction of this conductor in magnetic measure, expressed in centimetres; C the capacity of either of its ends in electrostatic measure (and therefore expressed also in centimetres); and finally A the velocity of light in centimetre/seconds. Then, assuming that the resistance is small, $T = \pi\sqrt{PC}/A$. In our experiments the capacity of the ends of the conductor consisted mainly of the spheres attached to them. We shall therefore not be far wrong if we take C as being the radius of either of these spheres, or put $C = 15$ cm.[3] As regards the coefficient of self-induction P, it was that of a straight wire, of diameter $d = 0{\cdot}5$ cm., and of which the length L was 150 cm. when resonance occurred. Calculated by Neumann's formula $P = \iint \cos \epsilon / r \, ds \, ds'$, the value of P for such a wire is

[1] [See IV., p. 63.] [2] Lorenz, *Wied. Ann.* 7, p. 161, 1879.
[3] [See Note 6 at end of book.]

$2L\{\log \text{nat } (4L/d) - 0{\cdot}75\}$ and therefore in our experiments $P = 1902$ cm.

At the same time we know that it is not certain whether Neumann's formula is applicable to open circuits. The most general formula, as given by v. Helmholtz, contains an undetermined constant k, and this formula is in accordance with the known experimental data. Calculated according to the general formula, we get for a straight cylindrical wire of length L and diameter d the value $P = 2L\{\log \text{nat } (4L/d) - 0{\cdot}75 + \frac{1}{2}(1-k)\}$. If in this we put $k = 1$, we arrive at Neumann's value. If we put $k = 0$, or $k = -1$, we obtain values which correspond to Maxwell's theory or Weber's theory. If we assume that one at any rate of these values is the correct one, and therefore exclude the assumption that it may have a very large negative or positive value, then the true value of k is not of much moment. For the coefficients calculated with these various values of k differ from each other by less than one-sixth of their value; and so if the coefficient 1902 does not exactly correspond to a length of wire of 150 cm., it does correspond to a length of our primary conductor not differing greatly therefrom. From the values of P and C it follows that the length $\pi\sqrt{CP}$ is 531 cm. This is the distance through which light travels in the time of an oscillation, and is at the same time the wave-length of the electromagnetic waves which, according to Maxwell's view, are supposed to be the external effect of the oscillations. From this length it follows that the period of oscillation itself (T) is 1·77 hundred-millionths of a second; thus the statement which we made in the beginning as to the order of magnitude of the period is justified.

Let us now turn our attention to what the theory can tell us as to the ratio of damping of the oscillations. In order that oscillations may be possible in the open circuit, its resistance must be less than $2A\sqrt{P/C}$. For our primary conductor $\sqrt{P/C} = 11{\cdot}25$: now since the velocity A is equal to 30 earth-quadrant/seconds, or to 30 ohms, it follows that the limit for r admissible in our experiment is 676 ohms. It is very probable that the true resistance of a powerful discharge lies below this limit, and thus from the theoretical point of view there is no contradiction of our assumption of oscillatory

motion. If the actual value of the resistance lies somewhat below this limit, the amplitude of any one oscillation would bear to the amplitude of that immediately following the ratio of 1 to $e^{-(rT/2P)}$. The number of oscillations required to reduce the amplitude in the ratio of 2·71 to 1 is therefore equal to $2P/rT$ or $2A\sqrt{P/C}/\pi r$. It therefore bears to 1 the same ratio that $1/\pi$ of the calculated limiting value bears to the actual value of the resistance, or the same ratio as 215 ohms to r. Unfortunately we have no means of even approximately estimating the resistance of a spark-gap. Perhaps we may regard it as certain that this resistance amounts to at least a few ohms, for even the resistance of strong electric arcs does not fall below this. It would follow from this that the number of oscillations we have to consider should be counted by tens and not by hundreds or thousands.[1] This is in complete accordance with the character of the phenomena, as has already been pointed out at the end of the preceding section. It is also in accordance with the behaviour of the very similar oscillatory discharges of Leyden jars, in which case the oscillations of perceptible strength are similarly limited to a very small number.

In the case of purely metallic secondary circuits the conditions are quite different from those of the primary currents to which we have confined our attention. In the former a disturbance would, according to theory, only come to rest after thousands of oscillations. There is no good reason for doubting the correctness of this result; but a more complete theory would certainly have to take into consideration the reaction upon the primary conductor, and would thus probably arrive at higher values for the damping of the secondary conductor as well.

Finally, we may raise the question whether the induction-effects of the oscillations which we have observed were of the same order as those which theory would lead us to expect, or whether there is here any appearance of contradiction between the phenomena themselves and our interpretation of them. We may answer the question by the following considerations:— We observe, in the first place, that the maximum value of the electromotive force which the oscillation induces in its own

[1] [See Note 7 at end of book.]

circuit must be very nearly equal to the maximum difference of potential at the ends, for if the oscillations were not damped, there would exist complete equality between the two magnitudes; inasmuch as the potential difference of the ends and the electromotive force of induction would in that case be in equilibrium at every instant. Now in our experiments the potential difference between the ends was of a magnitude corresponding to a sparking distance of 7-8 mm., and any such sparking distance fixes the value of the greatest inductive effect of the oscillation in its own path. We observe, in the second place, that at every instant the induced electromotive force in the secondary circuit bears to that induced in the primary circuit the same ratio as the coefficient of mutual induction p between the primary and secondary circuit bears to the coefficient of self-induction P of the primary circuit. There is no difficulty in calculating according to known formulæ the approximate value of p for our resonance-experiments. It was found to vary in the different experiments between one-ninth and one-twelfth of P. From this we may conclude that the maximum electromotive force which our oscillation excites in the secondary circuit should be of such strength as to give rise to sparks of $^1/_2$ to $^2/_3$ mm. in length. And accordingly the theory allows us, on the one hand, to expect visible sparks in the secondary circuit under all circumstances, and, on the other hand, we see that we can only explain sparks of several millimetres in length by assuming that several successive inductive effects strengthen each other. Thus from the theoretical side as well we are compelled to regard the phenomena which we have observed as being the results of resonance.

Further application of theory to these phenomena can only be of service when we shall have succeeded by some means in determining the period of oscillation directly. Such measurement would not only confirm the theory but would lead to an extension of it. The purpose of the present research is simply to show that even in short metallic conductors oscillations can be induced, and to indicate in what manner the oscillations which are natural to them can be excited.

III

FROM HERR W. VON BEZOLD'S PAPER: "RESEARCHES ON THE ELECTRIC DISCHARGE — PRELIMINARY COMMUNICATION"

(*Poggendorff's Annalen*, **140**, p. 541. *Berichte der Bayrischen Akad. d. Wissensch.*, 1870.)

As we must extract that part of v. Bezold's work which alone concerns us here, and remove it from its context, it may be advisable to preface it by some explanation.

Von Bezold's researches start with observations on Lichtenberg dust-figures. Herr von Bezold had noticed that under certain conditions the characters of the positive and negative figures appeared to undergo some modification and to resemble each other more nearly; so much so that the negative figure, for example, might be taken for positive. The first deviation from the normal character always presents itself in the form of a small positive figure appearing in the centre of the negative figure, or a small negative figure in the centre of the positive figure. It appeared that the more complicated figures always owed their origin to a spark associated with an alternating discharge, while the simple figures were produced by simple discharges. In every complicated figure an alternating discharge to a certain extent registered itself; and from the appearance of the figure the alternating character of the discharge and the direction of its first outburst could be directly recognised. We can, therefore, make use of the Lichtenberg figures in investigating the discharge.

Von Bezold produced the Lichtenberg figures in the following way:—A horizontal plate of well-insulating glass was

covered underneath with tinfoil, and this was connected to earth. On the upper side of the plate was placed the point of a vertical knitting-needle, to which the discharge was led. The needle was then removed, the plate dusted, and the figure produced was examined. If this arrangement (which will be called the test-plate) is inserted directly in the discharge-circuit of a Leyden jar or of the conductor of an electrical machine, it naturally makes it impossible for an alternating discharge, and in general for any complete discharge, to occur. Hence the test-plate can only be inserted in a shunt to the actual discharge which is being investigated.

While Herr von Bezold was carrying out this method—using the discharge to earth of the conductor of his electrical machine as the principal discharge—he at once came upon certain very remarkable phenomena. Positive figures made their appearance where negative ones were to be expected; large figures appeared where one might have supposed there would be small ones, and *vice versa*. Ohm's law did not explain the facts; it appeared as if the electricity in motion had the power of carrying with it neighbouring electrifications, as if "phenomena occurred in electrical disturbances similar to those which are observed in the motion of fluids under the name of aspiration-phenomena." Of course many of the details were bound to remain unexplained at first. We now quote Herr von Bezold's own words:

"These peculiar observations led to further researches on the division of electrical discharge-currents.

"Here, again, alternating discharges gave more constant results than simple ones; care was therefore taken to provide always a suitable return-conductor. The above experiments show that a simple wire cannot be employed for this purpose; hence the induction-coil of the Ruhmkorff R was used as a return-conductor.

"If now the electrical machine Q was slowly turned until a spark passed at F, complex positive figures appeared with great regularity on the plate at A.

"If a portion of the current was diverted along a short wire D, and this branch-current led on to the plate in the same way

by a conductor B, there appeared, as might be expected, two precisely similar figures. If, on the other hand, the branch-wire was of considerable length (somewhat more than 1 metre), there was a decided difference in size between the figures. As soon as the length of the wire exceeded this limit, the figure at B was always larger than at A, even when the branch had its origin quite near the end of the conductor (1 cm. above the plate). The difference in size between the two figures became more striking as the length of the branch-wire D was increased, until finally, when D was equal to 6·4 metres, and F (the length of the spark-gap) was equal to 4·0 mm., the figure at A was reduced to a small star, and often was altogether absent.

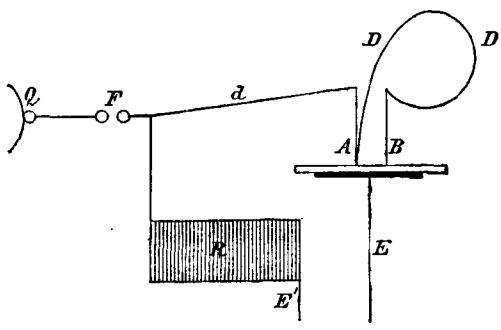


Fig. 12.

"This experiment obviously shows that Ohm's law only holds good for stationary currents, and not for electrical discharges, as indeed all theoretical investigations have shown. For whereas no electricity reaches the plate through the short branch A, it rushes, at any rate apparently, along the path through the wire D which is several hundred times longer.

"If the wire D is still further lengthened there is (within fairly wide limits) no change at first to be observed; only when the length has been about doubled does the figure at A again become larger, until with still greater lengths the difference in size between the two figures again completely disappears. It made no difference whether the wire used was thick or thin, whether it was a good or bad conductor, nor yet whether it was tightly stretched or curved. I have not yet experimented with wire spirals.

"On account of the complete novelty of the phenomena it appeared to me of interest to investigate the behaviour of the

wire D at different places. For this purpose a modification shown in Fig. 13 was made. On the plate were placed the conductors A, B, C, which were connected together by two wires D and D'. If the length of these wires is chosen so as to produce as large as possible a figure at C, and, on the other hand, as small as possible a figure at A, then the figure at B is larger than that at A and smaller than that at C. If the wires are longer the sizes of the figures A and C are more nearly equal, whereas, when the ratio $D:D'$ is suitably chosen, B becomes quite small and even disappears altogether. When the sparking distance was 4·3 mm. and the length AF was equal to 50 cm., $D = 6{\cdot}2$ m. and $D' = 8{\cdot}1$ m., the figures at A and C were large, whereas only a small star appeared at B.

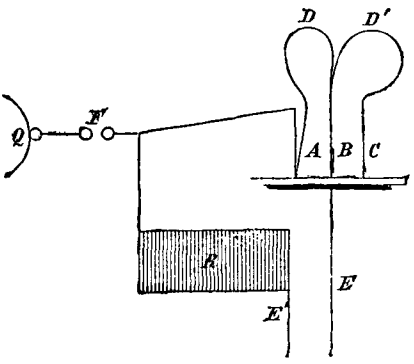


Fig. 13.

"If any one of the conductors is lifted away from the plate, the figures at the remaining conductors are not in the least altered thereby.

"This experiment teaches us a new fact, viz. that by simply connecting a conductor with a wire which has a blind end, we can produce important modifications in the figure which is formed by that conductor, and may even cause it to disappear. The most instructive way of performing the experiment is to bring near the conductor A a second spark-micrometer f (Fig. 14), one knob of which is connected with A while the other leads to the wire D. If now the distance between the knobs of the micrometer f is at first made large and then gradually diminished, it is seen that at the instant when the spark passes at f the figure at A becomes altered or disappears. But if we consider that in the case of an alternating discharge the wire forthwith becomes completely discharged, it follows that in such a case electricity is first driven to the outer end of the

wire *D*, and then immediately back again; that, in fact, we have here disturbances which are entirely comparable with reflection.

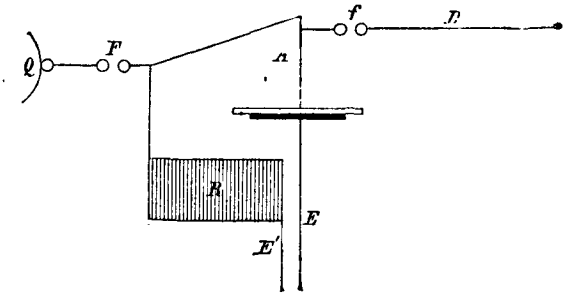


Fig. 14.

"This consideration leads to an hypothesis respecting the peculiar changes of size which the dust-figures undergo when the discharge branches as above described.

"If electric waves are impelled along a wire and forced to return along the same path after reflection at the end, the advancing and reflected waves would interfere and so give rise to phenomena analogous to those observed in organ-pipes. The observations already described point distinctly to such an analogy, and we may venture to compare the positions of the wire in which maximum and minimum figures appear with the antinodes and nodes.

"The hypothesis that the phenomena under discussion are due to interference gains additional support from the fact that it is only with alternating currents that the experiment is a decided success; differences in size between the various figures are indeed observed with simple discharges too, but not nearly to the same extent.

"In connection with the above experiments, a small modification was tried, which in turn became the starting-point for fresh researches:—

"If the end of the wire *D* (Fig. 12) is brought back to and connected with the first conductor *A*, as shown in the accompanying diagram, the figure can again be made to disappear by suitably choosing the length of the wire. Strictly speaking, this experiment was the starting-point of all the others already described; but I have deferred the description of it until now

because it does not help us much to understand the above experiments. For my own part, I at first believed that I had

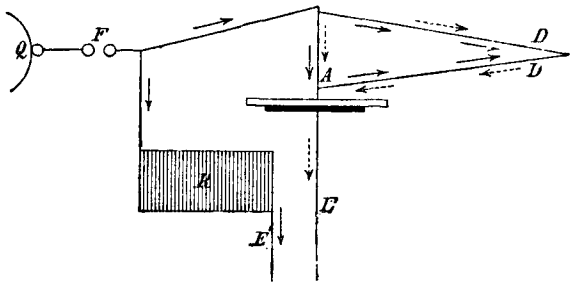


Fig. 15.

found in it an analogue to Savart's interference-experiment for sound-waves, and pictured the currents to myself as moving in the direction of the dotted arrows. This view was upset by the experiments in which the wire with a blind end was used, as well as by the fact that the distance between the two points of divergence on *A* exercised no decided effect on the result. In order to remove all doubt on this score I introduced a spark-gap into the branch *D* at various places in turn. The knobs of this second micrometer were only 0·01 to 0·03 mm. apart. I reasoned thus: If the current enters the wire from both ends there must be some point on the wire at which the two trains of waves meet. When the spark-gap is exactly at this point, the potential on the two balls must reach the same value simultaneously, and there can be no cause for the production of sparks at this point; whereas, at all other points, sparking was to be expected.

"And, as a matter of fact, the sparking ceased when the micrometer was introduced in the middle of a branch, and began again as soon as it was removed from this point a few decimetres on either side. It is thus proved that the direction of the current is as indicated by the continuous arrows; and, on the other hand, the small retardation which the electric discharge-current suffers while traversing a few decimetres of wire is here made evident.

"I now endeavoured chiefly to find out under what experimental conditions this retardation was most effectively shown. I found it best to use the direct discharge of a Ruhmkorff coil,

as shown in the diagram (Fig. 16). The inducing current was provided by a Grove cell, and the sparking distance F in the

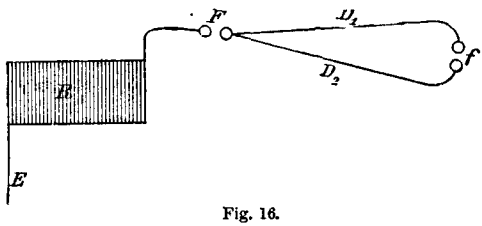


Fig. 16.

spark-micrometer was made about 2 mm., inasmuch as neither larger nor smaller spark-gaps gave such good results.

"Under these circumstances it sufficed for the production of the spark if one of the wires D was only one decimetre longer than the other. On the contrary, no spark ever appeared if they were of the same length. It can, however, be made to appear at once if the symmetry of the two branches is upset by placing the knob of a Leyden jar in contact with one of the wires.

"In this experiment, again, the material and thickness of the wires had not the slightest effect. Whether I used a silvered copper wire of 0·06 mm. diameter, or an iron wire of 0·23 mm., or a copper wire of 0·80 mm. diameter, there was never any spark as long as the two wires were of equal length.

"Thus the velocity of electricity for all (stretched)[1] wires is the same.

"The experiment is still not easily visible in the form above described, for one can only work with a small spark-gap in the auxiliary micrometer f. I therefore endeavoured to modify it in such a way as would admit of its being exhibited in a lecture-room.

"Trials with small Geissler tubes gave no definite result. On the other hand, the retardation can be very well shown in the following way, provided the difference of path amounts to a few metres (Fig. 17):—

"Let a (negative) discharge, preferably from a Ruhmkorff coil, be divided as above directly beyond the spark-micrometer into two branches. Let one of them be connected with the coating of a completely insulated test-plate, while the other is

[1] Wires wound spirally would probably have given a different result.

led by the conductor A on to the upper uncovered surface. Under these conditions a positive figure, or a negative one, or no figure at all, may appear on the upper surface according as the upper branch is larger, smaller, or of the same length as the lower. Indeed, we can predict what results the experiments must give if the supposition is correct that they are caused by time-differences. For if we recollect that the effect of leading

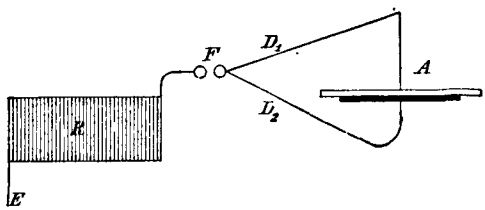


Fig. 17.

positive electricity on to the plate is the same as the effect of removing negative electricity, we can understand how a positive discharge gives rise to a positive figure if the electricity reaches the point of the conductor before it reaches the coating, *i.e.* if D_1 is shorter than D_2. If, on the other hand, the discharge arrives first at the coating, the induced electricity traverses the conductor in the opposite sense, and hence a negative figure must make its appearance upon the pane as soon as D_2 is shorter than D_1. In the course of the disturbance the induced charge in the wire D_1 must meet the electricity arriving directly from F, and hence a complex character will be impressed upon the figure.

"Between these two dispositions, which give opposite results, there obviously must be another in which no figure can be produced, because there is no reason why the one kind or the other should appear. This must be the case when the electricity arrives simultaneously from both sides, *i.e.* when D_1 and D_2 are of the same length.[1]

"The experiments entirely coincided with these theoretical predictions. With either kind of electricity figures of both kinds can be obtained if the lengths of the wires are rightly chosen.

[1] A small difference of length in favour of the upper wire may occur here, inasmuch as the electricity arriving from below has to spread itself out over the whole coating.

"It is true that this assertion may appear incorrect to many who may try the experiment under conditions which are not quite favourable; excepting the one case in which, on account of the exact equality of the two paths, no figure appears. For it may happen that at first sight all the figures appear to be positive under whatever conditions one works and with either kind of electricity.

"The reason is simply that the complex negative figures in this case belong to the class which already bear a strong positive character, and can scarcely be recognised as negative even after thorough experience of them.

"But the considerable difference in size which results from a change of pole amply suffices to remove at once any doubt respecting the real nature of the figures, and to show the accordance between the experiments and the theoretical predictions.

"To sum up, the following results were obtained:—

"1. If, after springing across a spark-gap, an electric discharge has before it two paths to earth, one short and the other long, and separated by a test-plate, the discharge-current splits up, so long as the sparking distance is small. But when it is larger the electricity rushes solely along the shorter path, carrying with it out of the other branch electricity of the same sign.

"2. If a series of electric waves is sent along a wire which is insulated at the end, the waves are reflected at the end, and the phenomena which accompany this process in the case of alternating discharges appear to be caused by interference between the advancing and reflected waves.

"3. An electric discharge traverses wires of equal lengths in equal times, whatever may be the material of which these wires consist."

IV

ON AN EFFECT OF ULTRA-VIOLET LIGHT UPON THE ELECTRIC DISCHARGE

(*Sitzungsberichte d. Berl. Akad. d. Wiss.*, June 9, 1887. *Wiedemann's Ann.* **31**, p. 983.)

In a series of experiments on the effects of resonance between very rapid electric oscillations which I have carried out and recently published,[1] two electric sparks were produced by the same discharge of an induction-coil, and therefore simultaneously. One of these, the spark *A*, was the discharge-spark of the induction-coil, and served to excite the primary oscillation. The second, the spark *B*, belonged to the induced or secondary oscillation. The latter was not very luminous; in the experiments its maximum length had to be accurately measured. I occasionally enclosed the spark *B* in a dark case so as more easily to make the observations; and in so doing I observed that the maximum spark-length became decidedly smaller inside the case than it was before. On removing in succession the various parts of the case, it was seen that the only portion of it which exercised this prejudicial effect was that which screened the spark *B* from the spark *A*. The partition on that side exhibited this effect, not only when it was in the immediate neighbourhood of the spark *B*, but also when it was interposed at greater distances from *B* between *A* and *B*. A phenomenon so remarkable called for closer investigation. The following communication contains the results which I have been able to establish in the course of the investigation:—

[1] See II., p. 29.

1. The phenomenon could not be traced to any screening effect of an electrostatic or electromagnetic nature. For the effect was not only exhibited by good conductors interposed between *A* and *B*, but also by perfect non-conductors, in particular by glass, paraffin, ebonite, which cannot possibly exert any screening effect. Further, metal gratings of coarse texture showed no effect, although they act as efficient screens.

2. The fact that both sparks *A* and *B* corresponded with synchronous and very rapid oscillations was immaterial. For the same effect could be exhibited by exciting two simultaneous sparks in any other way. It also appeared when, instead of the induced spark, I used a side-spark (this term having the same significance as in my earlier paper). It also appeared when I used as the spark *B* a side-discharge (according to Riess's terminology), such as is obtained by connecting one pole of an induction-coil with an insulated conductor and introducing a spark-gap. But it can best and most conveniently be exhibited by inserting in the same circuit two induction-coils with a common interruptor, the one coil giving the spark *A* and the other the spark *B*. This arrangement was almost exclusively used in the subsequent experiments. As I found the experiment succeed with a number of different induction-coils, it could be carried out with any pair of sets of apparatus at pleasure. At the same time it will be convenient to describe the particular experimental arrangement which gave the best results and was most frequently used. The spark *A* was produced by a large Ruhmkorff coil (*a*, Fig. 18), 52 cm. long and 20 cm. in diameter, fed by six large Bunsen cells (*b*) and provided with a separate mercury-break (*c*). With the current used it could give sparks up to 10 cm. long between point and plate, and up to about 3 cm. between two spheres. The spark generally used was one of 1 cm. length between the points of a common discharger (*d*). The spark *B* was produced by a smaller coil (originally intended for medical use) of relatively greater current-strength, but having a maximum spark-length of only $^1/_2$-1 cm. As it was here introduced into the circuit of the larger coil, its condenser did not come into play, and thus it only gave sparks of 1-2 mm. length. The

sparks used were ones about 1 mm. long between the nickel-plated knobs of a Riess spark-micrometer (f), or between brass knobs of 5 to 10 cm. diameter. When the apparatus thus arranged was set up with both spark-gaps parallel and not too far apart, the interruptor set going, and the spark-micrometer drawn out just so far as to still permit sparks to pass regularly, then on placing a plate (p) of metal, glass, etc., between the two spark-gaps d and f, the sparks are extinguished immediately and completely. On removing the plate they immediately reappear.

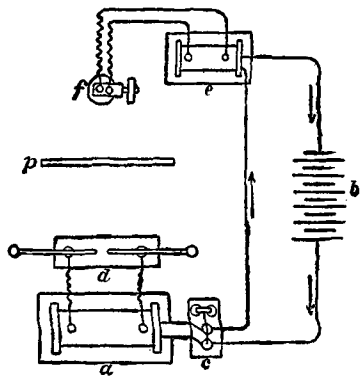


Fig. 18.

3. The effect becomes more marked as the spark A is brought nearer to the spark B. The distance between the two sparks when I first observed the phenomenon was $1\frac{1}{2}$ metres, and the effect is, therefore, easily observed at this distance. I have been able to detect indications of it up to a distance of 3 metres between the sparks. But at such distances the phenomenon manifests itself only in the greater or less regularity of the stream of sparks at B; at distances less than a metre its strength can be measured by the difference between the maximum spark-length before and after the interposition of the plate. In order to indicate the magnitude of the effect I give the following, naturally rough, observations which were obtained with the experimental arrangement shown in Fig. 18:—

Distance between the Sparks in cm.	Length of Spark B in mm. before and after inserting the Plate.		Difference.
∞	0·8	0·8	0
50	0·9	0·8	0·1
40	1·0	0·8	0·2
30	1·1	0·8	0·3
20	1·3	0·8	0·5
10	1·5	0·8	0·7
5	1·6	0·8	0·8
2	1·8	0·8	1·0

It will be seen that, under certain conditions, the sparking distance is doubled by removing the plate.

4. The observations given in the table may also be adduced as proofs of the following statement which the reader will probably have assumed from the first. The phenomenon does not depend upon any prejudicial effect of the plate on the spark *B*, but upon its annulling a certain action of the spark *A*, which tends to increase the sparking distance. When the distance between the sparks *A* and *B* is great, if we so adjust the spark-micrometer that sparks no longer pass at *B*, and then bring the spark-micrometer nearer to *A*, the stream of sparks in *B* reappears; this is the action. If we now introduce the plate, the sparks are extinguished; this is the cessation of the action. Thus the plate only forms a means of exhibiting conveniently and plainly the action of the spark *A*. I shall in future call *A* the active spark and *B* the passive spark.

5. The efficiency of the active spark is not confined to any special form of it. Sparks between knobs, as well as sparks between points, proved to be efficient. Short straight sparks, as well as long jagged ones, exhibited the effect. There was no difference of any importance between faintly luminous bluish sparks and brilliant white ones. Even sparks 2 mm. long made their influence felt to considerable distances. Nor does the action proceed from any special part of the spark; every part is effective. This statement can be verified by drawing a glass tube over the spark-gap. The glass does not allow the effect to pass through, and so the spark under these conditions is inactive. But the effect reappears as soon as a short bit of the spark is exposed at one pole or the other, or in the middle. I have not observed any influence due to the metal of the pole. And in arranging the experiment it is not of importance that the active spark should be parallel to the passive one.

6. On the other hand, the susceptibility of the passive spark to the action is to a certain extent dependent upon its form. I could detect no susceptibility with long jagged sparks between points, and but little with short sparks

between points. The effect was best displayed by sparks between knobs, and of these most strikingly by short sparks. It is advisable to use for the experiments sparks 1 mm. long between knobs of 5-10 mm. diameter. Still I have distinctly recognised the effect with sparks 2 cm. long. Perhaps the absolute lengthening which such sparks experience is really as great as in the case of shorter sparks, but at all events the relative increase in length is much smaller; and hence the effect disappears in the differences which occur between the single discharges of the coil. I have not discovered any perceptible influence due to the material of the pole. I examined sparks between poles of copper, brass, iron, aluminium, tin, zinc, and lead. If there was any difference between the metals with respect to the susceptibility of the spark, it appeared to be slightly in favour of the iron. The poles must be clean and smooth; if they are dirty, or corroded by long use, the effect is not produced.

7. The relation between the two sparks is reciprocal. That is to say, not only does the larger and stronger spark increase the spark-length of the smaller one, but conversely the smaller spark has the same effect upon the spark-length of the larger one. For example, using the same apparatus as before, let us adjust the spark-micrometer so that the discharge in it passes over regularly; but let the discharger be so adjusted that the discharges of the large coil just miss fire. On bringing the spark-micrometer nearer we find that these discharges are again produced; but that on introducing a plate the action ceases. For this purpose the spark of the large coil must naturally be fairly sensitive; and, inasmuch as long sparks are less sensitive, the effect is not so striking. If both coils are just at the limit of their sparking distance complications arise which have probably no connection with the matter at present under discussion.[1] One frequently has occasion to notice a long spark being started by other ones which are much smaller, and in part this may certainly be ascribed to the action which we are investigating. When the discharge of a coil is made to take place between knobs, and the knobs are drawn apart until the sparks cease, then it

[1] [See Note 8 at end of book.]

is found that the sparking begins again when an insulated conductor is brought near one of the knobs so as to draw small side-sparks from it. I have proved to my entire satisfaction that the side-discharges here perform the function of an active spark in the sense of the present investigation. It is even sufficient to touch one of the knobs with a non-conductor, or to bring a point somewhat near it, in order to give rise to the same action. It appears at least possible that the function of an active spark is here performed by the scarcely visible side-discharges over the surface of the non-conductor and of the point.

8. The effect of the active spark spreads out on all sides in straight lines and forms rays exactly in accordance with the laws of the propagation of light. Suppose the axes of both of the sparks used to be placed vertically, and let a plate with a vertical edge be pushed gradually from the side in between the sparks. It is then found that the effect of the active spark is stopped, not gradually, but suddenly, and in a definite position of the plate. If we now look along the edge of the plate from the position of the passive spark, we find that the active spark is just hid by the plate. If we adjust the plate with its edge vertical between the two sparks and slowly remove it sideways, the action begins again in a definite position, and we now find that, from the position of the passive spark, the active spark has just become visible beyond the edge of the plate. If we place between the sparks a plate with a small vertical slit and move it backwards and forwards, we find that the action is only transmitted in one perfectly definite position, namely, when the active spark is visible through the slit from the position of the passive spark. If several plates with such slits are interposed behind each other, we find that in one particular position the action passes through the whole lot. If we seek these positions by trial, we end by finding (most easily, of course, by looking through) that all the slits lie in the vertical plane which passes through the two sparks. If at any distance from the active spark we place a plate with an aperture of any shape, and by moving the active spark about fix the limits of the space within which the action is exerted, we

obtain as this limit a conical surface determined by the active spark as apex and by the limits of the aperture. If we place a small plate in any position in front of the active spark we find, by moving the passive spark about, that the plate stops the action of the active spark within exactly the space which it shelters from its light. It scarcely requires to be explained that the action is not only annulled in the shadows cast by external bodies, but also in the shadows of the knobs of the passive spark. In fact, if we turn the latter so that its axis remains in the plane of the active spark, but is perpendicular to it instead of being parallel, the action immediately ceases.

9. Most solid bodies hinder the action of the active spark, but not all; a few solid bodies are transparent to it. All the metals which I tried proved to be opaque, even in thin sheets, as did also paraffin, shellac, resin, ebonite, and india-rubber; all kinds of coloured and uncoloured, polished and unpolished, thick and thin glass, porcelain, and earthenware; wood, pasteboard, and paper; ivory, horn, animal hides, and feathers; lastly, agate, and, in a very remarkable manner, mica, even in the thinnest possible flakes. Further investigation of crystals showed variations from this behaviour. Some indeed were equally opaque, *e.g.* copper sulphate, topaz, and amethyst; but others, such as crystallised sugar, alum, calc-spar, and rock-salt, transmitted the action, although with diminished intensity; finally, some proved to be completely transparent, such as gypsum (selenite), and above all rock-crystal, which scarcely interfered with the action even when in layers several centimetres thick. The following is a convenient method of testing:—The passive spark is placed a few centimetres away from the active spark, and is brought to its maximum length. The body to be examined is now interposed. If this does not stop the sparking the body is very transparent. But if the sparking is stopped, the spark-gap must be shortened until it comes again into action. An opaque substance is now interposed in addition to the body under investigation. If this stops the sparking once more, or weakens it, then the body must have been at any rate partially transparent; but if the plate produces no further effect it must have been quite opaque. The influence of the interposed bodies increases with their thickness,

and it may properly be described as an absorption of the action of the active spark; in general, however, even those bodies which only act as partial absorbers, exert this influence even in very thin layers.

10. Liquids also proved to be partly transparent and partly opaque to the action. In order to experiment upon them the active spark was brought about 10 cm. vertically above the passive one, and between both was placed a glass vessel, of which the bottom consisted of a circular plate of rock-crystal 4 mm. thick. Into this vessel a layer, more or less deep, of the liquid was poured, and its influence was then estimated in the manner above described for solid bodies. Water proved to be remarkably transparent; even a depth of 5 cm. scarcely hindered the action. In thinner layers pure concentrated sulphuric acid, alcohol, and ether were also transparent. Pure hydrochloric acid, pure nitric acid, and solution of ammonia proved to be partially transparent. Molten paraffin, benzole, petroleum, carbon bisulphide, solution of ammonium sulphide, and strongly coloured liquids, *e.g.* solutions of fuchsine, potassium permanganate, were nearly or completely opaque. The experiments with salt solutions proved to be interesting. A layer of water 1 cm. deep was introduced into the rock-crystal vessel; the concentrated salt solution was added to this drop by drop, stirred, and the effect observed. With many salts the addition of a few drops, or even a single drop, was sufficient to extinguish the passive spark; this was the case with nitrate of mercury, sodium hyposulphite, potassium bromide, and potassium iodide. When iron and copper salts were added, the extinction of the passive spark occurred before any distinct colouring of the water could be perceived. Solutions of sal-ammoniac, zinc sulphate, and common salt [1] exercised an absorption when added in larger quantities. On the other hand, the sulphates of potassium, sodium, and magnesium were very transparent even in concentrated solution.

11. It is clear from the experiments made in air that some gases permit the transmission of the action even to con-

[1] According to my experiments a concentrated solution of common salt is a more powerful absorbent than crystallised rock-salt. This result is so remarkable as to require confirmation.

siderable distances. Some gases, however, are very opaque to it. In experimenting on gases a tube 20 cm. long and 2·5 cm. in diameter was interposed between the active and passive sparks; the ends of this tube were closed by thin quartz plates, and by means of two side-tubes any gas could at will be led through it. A diaphragm prevented the transmission of any action excepting through the glass tube. Between hydrogen and air there was no noticeable difference. Nor could any falling off in the action be perceived when the tube was filled with carbonic acid. But when coal-gas was introduced, the sparking at the passive spark-gap immediately ceased. When the coal-gas was driven out by air the sparking began again; and this experiment could be repeated with perfect regularity. Even the introduction of air with which some coal-gas had been mixed hindered the transmission of the action. Hence a much shorter stratum of coal-gas was sufficient to stop the action. If a current of coal-gas 1 cm. in diameter is allowed to flow freely into the air between the two sparks, a shadow of it can be plainly perceived on the side remote from the active spark, *i.e.* the action of this is more or less completely annulled. A powerful absorption like that of coal-gas is exhibited by the brown vapours of nitrous oxide. With these, again, it is not necessary to use the tube with quartz-plates in order to show the action. On the other hand, although chlorine and the vapours of bromine and iodine do exercise absorption, it is not at all in proportion to their opacity. No absorptive action could be recognised when bromine vapour had been introduced into the tube in sufficient quantity to produce a distinct coloration; and there was a partial transmission of the action even when the bromine vapour was so dense that the active spark (coloured a deep red) was only just visible through the tube.

12. The intensity of the action increases when the air around the passive spark is rarefied, at any rate up to a certain point. The increase is here supposed to be measured by the difference between the lengths of the protected and the unprotected sparks. In these experiments the passive spark was produced under the bell-jar of an air-pump between adjustable poles which passed through the sides of the bell-jar. A window

of rock-crystal was inserted in the bell-jar, and through this the action of the other spark had to pass. The maximum spark-length was now observed, first with the window open, and then with the window closed; varying air-pressures being used, but a constant current. The following table may be regarded as typical of the results:—

Air-pressure in mm. of Mercury.	Length in mm. of Spark with Window		Difference.
	Closed.	Open.	
760	0·8	1·5	0·7
500	0·9	2·3	1·4
300	1·0	3·7	2·7
100	2·0	6·2	4·2
80	very great	very great	undetermined.

It will be seen that as the pressure diminishes, the length of the spark which is not influenced only increases slowly; the length of the spark which is influenced increases more rapidly, and so the difference between the two becomes greater. But at a certain pressure the blue glow-light (*Glimmlicht*) spread over a considerable portion of the cathode, the sparking distance became very great, the discharge altered its character, and it was no longer possible to perceive any influence due to the active spark.

13. The phenomenon is also exhibited when the sparking takes place in other gases than air; and also when the two sparks are produced in two different gases. In these experiments the two sparks were produced in two small tubulated glass vessels which were closed by plates of rock-crystal and could be filled with different gases. The experiments were tried mainly because certain circumstances led to the supposition that a spark in any given gas would only act upon another spark in the same gas, and on this account the four gases—hydrogen, air, carbonic acid, and coal-gas—were tried in the sixteen possible combinations. The main conclusion arrived at was that the above supposition was erroneous. It should, however, be added that although there is no great difference in the efficiency of sparks when employed as active sparks in different gases, there is, on the other hand, a notable difference in their susceptibility when employed as passive sparks. Other things being equal, sparks in hydrogen experienced a perceptibly greater

increase in length than sparks in air, and these again about double the increase of sparks in carbonic acid and coal-gas. It is true that no allowance was made for absorption in these experiments, for its effect was not known when they were carried out; but it could only have been perceptible in the case of coal-gas.

14. All parts of the passive spark do not share equally in the action; it takes place near the poles, more especially near the negative pole.[1] In order to show this, the passive spark is made from 1 to 2 cm. long, so that the various parts of it can be shaded separately. Shading the anode has but a slight effect; shading the cathode stops the greater part of the action. But the verification of this fact is somewhat difficult, because with long sparks there is a want of distinctness about the phenomenon. In the case of short sparks (the parts of which cannot be separately shaded) the statement can be illustrated as follows:—The passive spark is placed parallel to the active one, and is turned to right and left from the parallel into the perpendicular position until the action stops. It is found that there is more play in one direction than in the other; the advantage being in favour of that direction in which the cathode is turned towards the active spark. Whether the effect is produced entirely at the cathode, or only chiefly at the cathode, I have not been able to decide with certainty.

15. The action of the active spark is reflected from most surfaces. From polished surfaces the reflection takes place according to the laws of regular reflection of light. In the preliminary experiments on reflection a glass tube, 50 cm. long and 1 cm. in diameter, was used; this tube was open at both ends, and was pushed through a large sheet of cardboard. The active spark was placed at one end so that its action could only pass the sheet by way of the tube. If the passive spark was now moved about beyond the other end of the tube it was affected when in the continuation of the tubular space and then only; but in this case a far more powerful action was exhibited than when the tube was removed and only the diaphragm retained. It was this latter phenomenon that suggested the use of the tube; of itself it indicates a reflection from

[1] [See Note 9 at end of book.]

the walls of the tube. The spark-micrometer was now placed to one side of the beam proceeding out of the tube, and was so disposed that the axis of the spark was parallel to the direction of the beam. The micrometer was now adjusted so that the sparking just ceased; it was found to begin again if a plane surface inclined at an angle of 45° to the beam was held in it so as to direct the beam, according to the usual law of reflection, upon the passive spark. Reflection took place more or less from glass, crystals, and metals, even when these were not particularly smooth; also from such substances as porcelain, polished wood, and white paper. I obtained no reflection from a well-smoked glass plate.

In the more accurate experiments the active spark was placed in a vertical straight line; at a little distance from it was a largeish plate with a vertical slit, behind which could be placed polished plane mirrors of glass, rock-crystal, and various metals. The limits of the space within which the action was exerted behind the slit were then determined by moving the passive spark about. These limits were quite sharp and always coincided with the limits of the space within which the image of the active spark in the mirror was visible. On account of the feebleness of the action these experiments could not be carried out with unpolished bodies; such bodies may be supposed to give rise to diffused reflection.

16. In passing from air into a solid transparent medium the action of the active spark exhibits a refraction like that of light; but it is more strongly refracted than visible light. The glass tube used in the reflection experiments served here again for the rougher experiments. The passive spark was placed in the beam proceeding out of the tube and at a distance of about 30 cm. from the end farthest from the active spark; immediately behind the opening a quartz-prism was pushed sideways into the beam with its refracting edge foremost. In spite of the transparency of quartz, the effect upon the passive spark ceased as soon as the prism covered the end of the tube. If the spark was then moved in a circle about the prism in the direction in which light would be refracted by the prism, it was soon found that there were places at which the effect was again produced. Now let the passive spark be fixed in the

position in which the effect is most powerfully exhibited; on looking from this point towards the tube through the prism the inside of the tube and the active spark at the end of it cannot be perceived; in order to see the active spark through the tube the eye must be shifted backwards through a considerable distance towards the original position of the spark. The same result is obtained when a rock-salt prism is used. In the more accurate experiments the active spark was again fixed vertically; at some distance from it was placed a vertical slit, and behind this a prism. By inserting a Leyden jar the active spark could be made luminous, and the space thus illuminated

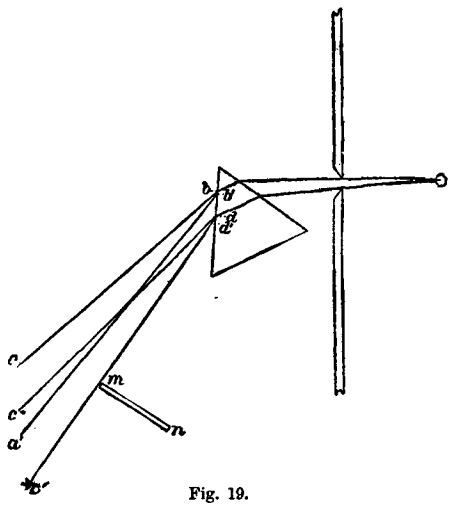


Fig. 19.

behind the prism could easily be determined. With the aid of the passive spark it was possible to mark out the limits of the space within which was exerted the action here under investigation. Fig. 19 gives (to a scale of $\frac{1}{2}$) the result thus obtained by direct experiment. The space $a\ b\ c\ d$ is filled with light; the space $a'\ b'\ c'\ d'$ is permeated by the action which we are considering. Since the limits of this latter space were not sharp, the rays $a'\ b'$ and $c'\ d'$ were fixed in the following way:—The passive spark was placed in a somewhat distant position, about c', at the edge of the tract within which the action was exerted. A screen $m\ n$ (Fig. 19) with vertical edge was then pushed in sideways until it stopped the action. The

position m of its edge then gave one point of the ray $c'\,d'$. In another experiment a prism of small refracting angle was used, and the width of the slit was made as small, and the spark placed as far from it as would still allow of the action being perceived. The visible light was then spread out into a short spectrum, and the influence of the active spark was found to be exerted within a comparatively limited region which corresponded to a deviation decidedly greater than that of the visible violet. Fig. 20 shows the positions of the rays as they were directly drawn where the prism was placed, r being the direction of the red, v of the violet, and w the direction in which the influence of the active spark was most powerfully exerted.

Fig. 20.

I have not been able to decide whether any double refraction of the action takes place. My quartz-prisms would not permit of a sufficient separation of the beams, and the pieces of calc-spar which I possessed proved to be too opaque.[1]

17. After what has now been stated, it will be agreed (at any rate until the contrary is proved) that the light of the active spark must be regarded as the prime cause of the action which proceeds from it. Every other conjecture which is based on known facts is contradicted by one or other of the experiments. And if the observed phenomenon is an effect of light at all it must, according to the results of the refraction-experiments, be solely an effect of the ultra-violet light. That it is not an effect of the visible parts of the light is shown by the fact that glass and mica are opaque to it, while they are transparent to these. On the other hand, the absorption-experiments of themselves make it probable that the effect is due to ultra-violet light. Water, rock-crystal, and the sulphates of the alkalies are remarkably transparent to ultra-violet light and to the action here investigated; benzole and allied substances

[1] [See Note 10 at end of book.]

are strikingly opaque to both. Again, the active rays in our experiments appear to lie at the outermost limits of the known spectrum. The spectrum of the spark when received on a sensitive dry-plate scarcely extended to the place at which the most powerful effect upon the passive spark was produced. And, photographically, there was scarcely any difference between light which had, and light which had not, passed through coal-gas, whereas the difference in the effect upon the spark was very marked. Fig. 21 shows the extent of some of the spectra taken. In *a* the position of the visible red is indicated by *r*, that of the visible violet by *v*, and that of the strongest effect upon the passive spark by *w*. The rest of the series give the photographic impressions produced—*b* after simply passing through air and quartz, *c* after passing through coal-gas, *d* after passing through a thin plate of mica, and *e* after passing through glass.

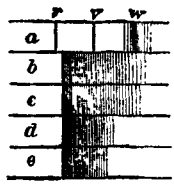


Fig. 21.

18. Our supposition that this effect is to be attributed to light is confirmed by the fact that the same effect can be produced by a number of common sources of light. It is true that the power of the light, in the ordinary sense of the word, forms no measure of its activity as here considered; and for the purpose of our experiments the faintly visible light of the spark of the induction-coil ren[illegible] the most powerful source of light. Let sparks from any indu[illegible]on-coil pass between knobs, and let the knobs be drawn so far apart that the sparks fail to pass; if now the flame of a candle be brought near (about 8 cm. off) the sparking begins again. The effect might at first be attributed to the hot air from the flame; but when it is observed that the insertion of a thin small plate of mica stops the action, whereas a much larger plate of quartz does not stop it, we are compelled to recognise here again the same effect. The flames of gas, wood, benzene, etc., all act in the same way. The non-luminous flames of alcohol and of the Bunsen burner exhibit the same effect, and in the case of the candle-flame the action seems to proceed more from the lower, non-luminous part than from the upper and luminous part. From a small hydrogen flame scarcely any effect could be obtained. The light from

platinum glowing at a white-heat in a flame, or through the action of an electric current, a powerful phosphorus flame burning quite near the spark, and burning sodium and potassium, all proved to be inactive. So also was burning sulphur; but this can only have been on account of the feebleness of the flame, for the flame of burning carbon bisulphide produced some effect. Magnesium light produced a far more powerful effect than any of the above sources; its action extended to a distance of about a metre. The limelight, produced by means of coal-gas and oxygen, was somewhat weaker, and acted up to a distance of half a metre; the action was mainly due to the jet itself: it made no great difference whether the lime-cylinder was brought into the flame or not. On no occasion did I obtain a decisive effect from sunlight at any time of the day or year at which I was able to test it. When the sunlight was concentrated by means of a quartz lens upon the spark there was a slight action; but this was obtained equally when a glass lens was used, and must therefore be attributed to the heating. But of all sources of light the electric arc is by far the most effective; it is the only one that can compete with the spark. If the knobs of an induction-coil are drawn so far apart that sparks no longer pass, and if an arc light is started at a distance of 1, 2, 3, or even 4 metres, the sparking begins again simultaneously, and stops again when the arc light goes out. By means of a narrow open[illegible]held in front of the arc light we can separate the violet [illegible] of the feebly luminous arc proper from that of the glowing [illegible]rbons; and we then find that the action proceeds chiefly fr[illegible]m the former. With the light of the electric arc I have repeated most of the experiments already described, *e.g.* the experiments on the rectilinear propagation, reflection, and refraction of the action, as well as its absorption by glass, mica, coal-gas, and other substances.

According to the results of our experiments, ultra-violet light has the property of increasing the sparking distance of the discharge of an induction-coil, and of other discharges. The conditions under which it exerts its effect upon such discharges are certainly very complicated, and it is desirable that the action should be studied under simpler conditions, and especially without using an induction-coil. In endeavouring

to make progress in this direction I have met with difficulties.[1] Hence I confine myself at present to communicating the results obtained, without attempting any theory respecting the manner in which the observed phenomena are brought about.

[1] [See Note 11 at end of book.]

V

ON THE ACTION OF A RECTILINEAR ELECTRIC OSCILLATION UPON A NEIGHBOURING CIRCUIT

(*Wiedemann's Annalen*, **34**, p. 155, 1888.)

In an earlier paper[1] I have shown how we may excite in a rectilinear unclosed conductor the fundamental electric oscillation which is proper to this conductor. I have also shown that such an oscillation exerts a very powerful inductive effect upon a nearly closed circuit in its neighbourhood, provided that the period of oscillation of the latter is the same as that of the primary oscillation. As I intended to make use of these effects in further researches, I examined the phenomenon in all the various positions which the secondary circuit could occupy with reference to the inducing current. The total inductive action of a current-element upon a closed circuit can be completely calculated by the ordinary methods of electromagnetics. Now since our secondary circuit is closed, with the exception of an exceedingly short spark-gap, I supposed that this total action would suffice to explain the new phenomena; but I found that in this I was mistaken. In order to arrive at a proper understanding of the experimental results (which are not quite simple), it is necessary to regard the secondary circuit also as being in every respect unclosed. Hence it is not sufficient to pay attention to the integral force of induction; we must take into consideration the distribution of the electromagnetic force along the various parts of the circuit: nor must the electrostatic force which proceeds from the charged ends of the oscillator be neglected. The reason of this is the rapidity with which

[1] See II., p. 29.

the forces in these experiments alter their sign. A slowly alternating electrostatic force would excite no sparks in our secondary conductor, even if its intensity were very great, since the free electricity of the conductor could distribute itself, and would distribute itself, in such a way as to neutralise the effect of the external force; but in our experiments the direction of the force alters so rapidly that the electricity has no time to distribute itself in this way.

For the sake of convenience I will first sketch the theory and then describe the phenomena in connection with it. It would indeed be more logical to adopt the opposite course; for the facts here communicated are true independently of the theory, and the theory here developed depends for its support more upon the facts than upon the explanations which accompany it.

The Apparatus

Before we proceed to develop the theory, we may briefly describe the apparatus with which the experiments were carried out, and to which the theory more especially relates. The primary conductor consisted of a straight copper wire 5 mm. in diameter, to the ends of which were attached spheres 30 cm. in diameter made of sheet-zinc. The centres of these latter were 1 metre apart. The wire was interrupted in the middle by a spark-gap $^3/_4$ cm. long; in this oscillations were excited by means of the most powerful discharges which could be obtained from a large induction-coil. The direction of the wire was horizontal, and the experiments were carried out only in the neighbourhood of the horizontal plane passing through the wire. This, however, in no way restricts the general nature of the experiments, for the results must be the same in any meridional plane through the wire. The secondary circuit, made of wire 2 mm. thick, had the form of a circle of 35 cm. radius which was closed with the exception of a short spark-gap (adjustable by means of a micrometer-screw). The change from the form used in the earlier experiments to the circular form was made for the following reason. Even the first experiments had shown that the spark-length was different at different points of the secondary conductor, even when the position of the conductor as a whole was not

altered. Now the choice of the circular form made it easily possible to bring the spark-gap to any desired position. This was most conveniently done by mounting the circle so that it could be rotated about an axis passing through its centre, and perpendicular to its plane. This axis was mounted upon various wooden stands in whatever way proved from time to time most convenient for the experiments.

With the dimensions thus chosen, the secondary circuit was very nearly in resonance with the primary. It was tuned more exactly by soldering on small pieces of sheet-metal to the poles so as to increase the capacity, and increasing or diminishing the size of these until a maximum spark-length was attained.

Analysis of the Forces acting on the Secondary Circuit

We shall assume that the electric force at every point varies as a simple periodic function of the time, changing its sense without changing its direction; we shall further assume that this variation has the same phase at all points. This is true at any rate in the neighbourhood of the primary conductor; and for the present we shall restrict our attention to points which lie near it. Any point on the secondary circuit is determined by its distance s measured from the spark-gap along the circle. We denote by Σ the component of the electric force which acts at any moment at the point s in the direction of the element $d\,s$ of the circle. Then Σ is a function of s which, after passing round the whole circumference S, returns to its original value. Σ can therefore be developed in circular functions, beginning thus—

$$\Sigma = A + B \cos 2\pi s/S + \ldots + B' \sin 2\pi s/S + \ldots$$

We shall neglect the higher terms. The effect of this will be that our results will only be approximately correct; in especial, weak sparks will be found to occur at places where our calculations indicate that the sparking should disappear. But for the present our experiments are not sufficiently accurate to justify us in paying any attention to these higher terms. Let us therefore consider more closely the terms which have been referred to.

In the first place, the force A acts in the same sense, and is of the same magnitude at all parts of the circle. A is independent of the electrostatic force; for the integral of the latter, taken all round the circuit, is zero. A corresponds to the total induced electromotive force. We know that this is measured by the change, per unit of time, in the number of magnetic lines of force which pass through the circle. If we regard the magnetic field in which the circle lies as being homogeneous, A will be proportional to the component of the magnetic force perpendicular to the plane of the secondary circuit. A will therefore vanish when the direction of the magnetic force lies in the plane of the secondary circuit. The force A gives rise to an oscillation the intensity of which is independent of the position of the spark-gap in the circle; we shall denote by a the spark-length which corresponds to this oscillation.

Turning now to the two other terms, we note, in the first place, that the force $B' \sin 2\pi s/S$ is not in a position to excite the fundamental oscillation of our circle. For it is completely symmetrical on both sides of the spark-gap; it acts in the same sense upon both poles, and therefore cannot produce any difference between them. The force $B \cos 2\pi s/S$ behaves otherwise. If we start from the spark-gap and divide the circle into four equal parts, we find that this force acts in the same direction in the two parts which lie opposite the spark-gap, and that here it powerfully excites the fundamental oscillation. It is true that B acts in an opposite direction in those parts which lie nearest the spark-gap; but the latter parts cannot here exert as powerful an effect. For since the current at the open ends of the circle must always be zero, the electricity cannot move with the same freedom near these ends as in the middle of the circle. To elucidate the meaning of this somewhat brief statement we may take as an illustration a string stretched between two fixed points. If the middle and outer parts of the string are acted upon by forces in opposite directions, the string as a whole will move as if acted upon by the former set of forces, and the fundamental note of the string will be produced if the alternations of these forces synchronise with this note. Thus the term $B \cos 2\pi s/S$ will excite the fundamental oscillation of our circle, and the direction of the oscillation will be

the same as if the force in the parts opposite the spark-gap were alone effective. Further, the intensity of the oscillation will be proportional to the quantity B. To find out what this quantity means, let us assume that the electric field in which the circle lies is approximately homogeneous. Let E denote the total electric force acting in this field, ω the angle which its direction makes with the plane of the secondary circle, and θ the angle which the projection of the force upon this plane makes with the straight line drawn from the centre to the spark-gap. Then $\Sigma = E \cos \omega \sin (2\pi s/S - \theta)$ approximately,[1] and therefore $B = -E \cdot \cos \omega \sin \theta$. Hence the value of B depends directly upon the total force; electrostatic as well as electromagnetic causes contribute towards it. B becomes zero when $\omega = 90°$, *i.e.* when the total force is perpendicular to the plane of the circle; and in this case it will be zero for all positions of the spark-gap in the circle. But B also becomes zero when $\theta = 0$, *i.e.* when the projection of the total force upon the plane of the circle coincides with the line drawn from the centre to the spark-gap. If in any given position of the circle we suppose the spark-gap to move round it, the angle θ alters, and corresponding alterations are produced in the value of B, in the intensity of the oscillation, and in the spark-length. Thus the spark-length, which corresponds to the second term of our series, can be approximately represented by the expression $\beta \sin \theta$.

The two terms which produce respectively the spark-lengths α and $\beta \sin \theta$ have always the same phase. Hence the induced oscillations have also the same phase, and their amplitudes have to be added together. Now inasmuch as the spark-lengths are approximately proportional to the total amplitudes, it follows that the spark-lengths have also to be added together. If in any given position of the circle we suppose the spark-gap to move round it, the spark-length must accordingly be represented by an expression of the form $\alpha + \beta \sin \theta$. Equal absolute values of this expression indicate equal spark-lengths, whatever the sign may be; for there is nothing in the spark-length which corresponds to direction of

[1] If the field is really homogeneous, then $A = 0$; and A will therefore be small when the field is approximately homogeneous. Nevertheless the force A may give rise to an oscillation of the same order of magnitude as that produced by the force B cos $2\pi s/S$.

oscillation. The absolute values of α and β could only be determined by a much more detailed development of the theory; but we have indicated the conditions upon which they depend, and this will be enough to enable us to understand the phenomena.

The Plane of the Secondary Circuit is Vertical

Let us now place our circle anywhere in the neighbourhood of the primary conductor, with its plane vertical and its centre in the horizontal plane which passes through the primary conductor. As long as the spark-gap lies in the horizontal plane, either on the one side or the other, we observe no sparks; but in other positions of the spark-gap we perceive sparks of greater or less length. The disappearance of the sparks occurs at two diametrically opposite points; it follows that the α of our formula is here always zero, and that θ becomes zero when the spark-gap lies in the horizontal plane. From this we draw the following conclusions:—In the first place, that the lines of magnetic force in the horizontal plane are everywhere vertical, and therefore form circles around the primary oscillation, as indeed is required by theory. Secondly, that at all points of the horizontal plane the lines of electric force lie in this plane itself, and therefore, that everywhere in space they lie in planes passing through the primary oscillation—which is also required by theory. If while the circle is in any one of the positions here considered, we turn it about its axis so as to remove the spark-gap out of the horizontal plane, the spark-length increases until the sparks arrive at the top or the bottom of the circle, in which positions they attain a length of 2-3 mm. It can be proved in various ways that the sparks thus produced correspond, as our theory requires, to the fundamental oscillation of our circle, and not, as might be suspected, to the first overtone. By making small alterations in the circle, for example, we can show that the oscillation which produces these sparks is in resonance with the primary oscillation; and this would not hold for the overtones. Again, the sparks disappear when the circle is cut at the points where it intersects the horizontal plane, although these points are nodes with respect to the first overtone.

If we now maintain the spark-gap at the highest point and turn the circle round about a vertical axis passing through its centre and the spark-gap, we find that during a complete revolution the sparks twice reach a maximum length and twice become zero or else very nearly disappear. Clearly the maximum positions are those in which the direction of the electric force lies in the plane of the circle ($\omega = 0$); whereas the minimum positions are those in which the direction of the electric force is perpendicular to this plane ($\omega = 90°$). Hence we now have a means of finding out the direction of the electric force at any point. I have investigated at a number of points the positions in which the sparks either become very short or completely disappear. In the lower part of Fig. 22

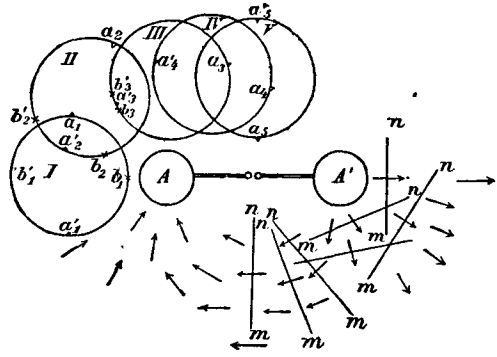


Fig. 22.

these are shown as taken directly from the experimental results. $A\,A'$ is the primary conductor. The straight lines $m\ n$ are the projections of the secondary conductor on the horizontal plane; but all the observed positions are not shown in the illustration. The short lines, normal to the lines $m\ n$, indicate the direction of the force. Since this force nowhere becomes zero, as we pass from the sphere A to the sphere A', it does not change its sign. Hence we may furnish these normals with arrow-heads, as has been done in the figure. With regard to this figure we remark:—

1. The distribution of the total force in the neighbourhood of the rectilinear oscillation is very similar to the distribution of the electrostatic force which proceeds from the ends of the

oscillation. Near the centre of the oscillation in especial the direction of the total force coincides with that of the electrostatic force; the opposing electromagnetic force must therefore be overpowered. Theory also indicates that in this neighbourhood the force of electromagnetic induction should be weaker than the electrostatic force.

2. Still we can quite easily recognise the effect of the electromagnetic induction. For the lines of force appear to a certain extent to be pushed away from the axis of the oscillation; in going from A to A' they make a wider circuit than they would if the electrostatic force alone were in operation.[1] The explanation of this is that the force of induction weakens the components of the electrostatic force which are parallel to the primary conductor, whereas they are without influence upon the components which are perpendicular to the primary conductor

The Plane of the Secondary Circle is Horizontal

We shall explain by reference to the upper half of Fig. 22 the phenomena which are observed when the plane of the secondary circle is horizontal. First suppose the circle to be brought into position *I*, in which its centre lies on the prolongation of the primary oscillation. After what has been already stated, we may at once conclude from purely geometrical considerations that the sparks will disappear when the spark-gap is at the points b_1 and b'_1; and also that maximum and equal spark-lengths should be observed at the points a_1 and a'_1.[1] In my experiments the lengths of these sparks were 2·5 mm.

Now let us shift the circle sideways into the position *II*. Here lines of magnetic force pass through the circle. The integral of the force of induction taken round the circle does not vanish; α is not zero. We may therefore expect to find that our expression $\alpha + \beta \sin \theta$, in which the value of α at first is small, will have (since we pay no regard to sign) two maxima of unequal value, viz. $\beta + \alpha$ and $\beta - \alpha$. These will occur when $\theta = 90°$, and the line joining them will be perpendicular to the direction of the electric force. These two maxima must be separated by two points at which no sparking

[1] The original drawing showed this more plainly than the reduced copy does.

occurs, and these points should lie near the smaller maximum. This agrees with the experimental results. For in the points b_2 and b'_2 we again find our null-points which have been drawn closer together; between these at a_2 and a'_2 are maximum spark-lengths, and that at a_2 is found to be 3·5 mm., while that at a'_2 is 2 mm. The line $a_2\, a'_2$ is very nearly perpendicular to the direction of the electric force. In order to complete our explanation we have yet to show that a_2 must correspond to the sum, and a'_2 to the difference of the actions. Let us consider the case in which the spark-gap lies at a_2. While the sphere A is positively charged, the total electric force in those parts of the circle which lie opposite to a_2 urges positive electricity in a direction away from A; it tends to move positive electricity in a circular direction, which in the case of our illustration would be the direction of the hands of a clock. Between the spheres A and A' the electrostatic force at the same time is directed from A towards A'; the force of induction which is always opposed to it is therefore, in the neighbourhood of the conductor, directed towards A, and everywhere in space is parallel to this direction. Now since this force in our circle acts more strongly in the neighbourhood of the primary oscillation than it does at a distance from the latter, it follows that this force also tends to set positive electricity in motion in a circular direction corresponding to that of the hands of a clock. Hence at a_2 both causes act in the same sense and so strengthen each other. Similarly it can be shown that at a'_2 they act in opposite senses and weaken each other. Thus the phenomenon is completely explained.

Now suppose our circle to be moved nearer the centre of the primary oscillation to *III.* Here the two points at which the spark is extinguished coincide into one. One maximum disappears; and opposite to a very extended tract of extinction a'_3 lies the second maximum with a spark-length of 4 mm. Here evidently $\alpha = \beta$, and the spark-length is represented by the formula $\alpha(1 + \sin\theta)$. The line $a_3\, a'_3$ is again perpendicular to the direction of the electric force. If we move the circle still nearer to the centre of the primary oscillation, α becomes greater than β. The expression $\alpha + \beta \sin\theta$ can no longer be zero for any value of θ, but it oscillates between a maximum value $\alpha + \beta$ and a minimum value $\alpha - \beta$.

Experiment also shows that in the positions under consideration there are no longer any points at which the sparks are extinguished; there are only maxima and minima. In position *IV* we have at a_4 a spark-length of 5·5 mm., and at a'_4 a length of 1·5 mm. In position *V* we have at a_5 a spark-length of 6 mm., at a'_5 the spark-length is 2·5 mm.,[1] and at intermediate points we have intermediate values. In passing over from position *III* to position *V*, the join $a\,a'$ turns sharply from a direction parallel to the primary current into a direction perpendicular to it; it therefore always remains approximately perpendicular to the direction of the electric force.

In the last-mentioned positions the sparks are mainly due to electromagnetic induction. Hence, in my first paper, I made no error in speaking of the phenomena in these positions as being electromagnetic effects. Nevertheless, the production of sparks even in these positions is completely independent of electrostatic causes only when we bring the spark-gap into the mean position between maximum and minimum, in which particular position $\beta \sin \theta$ becomes zero.

The Remaining Positions of the Secondary Circle

The positions which as yet have not been discussed, and in which the secondary circle is inclined to the horizontal plane, can be regarded as intermediate states between those which have already been described. In all such cases I have found the theory confirmed and have noticed no phenomenon which did not fit in with it. Let us consider one case only. Suppose the circle in the first place to lie in the horizontal plane and in position *V*, with the spark-gap turned towards the primary oscillation at a_5. Now let the circle be turned about a horizontal axis passing through its centre parallel to the primary oscillation, in such a way that the spark-gap rises. While the circle turns, the electric force is always at right angles to the straight line drawn from its centre to the spark-gap; thus θ is always equal to 90°. The value of β is approximately constant in all positions. But α varies approximately as the

[1] In these positions the secondary spark must, in order to avoid disturbing causes, be protected from the light of the primary spark.

cosine of the angle ϕ between the plane of the circle and the horizontal plane, since a is proportional to the number of lines of magnetic force cut by the circle. Thus if a_0 denote the value of a in the initial position, the value of a in any other position is $a_0 \cos \phi$, and it may therefore be expected that the relation between the spark-length and the angle ϕ may be given by the expression $a_0 \cos \phi + \beta$, where $a_0 > \beta$. Experiment confirms this. For as we raise the spark-gap the sparking distance steadily decreases from its initial value of 6 mm. and acquires at the highest point in its circuit a length of 2 mm. It then sinks farther in the second quadrant almost to zero, increases again to the smaller maximum of 2·5 mm., which occurs in the horizontal plane, again decreases, and after passing through the same stages in the reverse order it returns to its original value.

Let us suppose that in the course of the movements above described we hold the circle in the position in which the spark-gap is at its highest point. If now we raise the circle vertically as a whole, the sparks become weaker and ultimately they almost disappear; if we lower the circle vertically the sparking becomes more vigorous. But if under similar circumstances the spark-gap is at its lowest point, the effects are reversed. These results may be deduced by purely geometrical reasoning from what has been already stated.

The Forces at Greater Distances

We have already mentioned a method of ascertaining experimentally the direction of the total electric force at any point. There was no difficulty in extending the application of this method to greater distances, and there was all the more reason for making the experiment because the existing theories of electromagnetics differ widely in their views as to the distribution of the force in the neighbourhood of an unclosed current. We therefore place the plane of our circle in a vertical position, bring the spark-gap to the highest point, and by turning the circle about a vertical axis we try to ascertain in what position the sparks are longest and in what position they disappear or nearly disappear. But when we get to a distance of 1-1·5 metre from the primary oscillation,

we are met by an unexpected difficulty. For the maxima and minima lose their distinctness, except in particular positions, so that it becomes difficult to adjust the position of the circle for either; indeed, at certain places the differences between the spark-lengths during a revolution of the circle are so small that it becomes impossible to specify any definite direction of the force. Now observe that this difficulty again disappears when we pass beyond a distance of about 2 metres. Certainly the sparks are now very small and need to be observed in the dark and with the aid of a lens; but they disappear sharply in a definite position of the circle, and are strongest in a position at right angles to this. When the distance is further increased the spark-length only diminishes slowly. I have not been able to decide the farthest distance at which they could be observed. When I placed the primary conductor in one corner of a large lecture-room 14 metres long and 12 metres broad, the sparks could be perceived in the farthest parts of the room; the whole room seemed filled with the oscillations of the electric force. It is true that in the neighbouring rooms the action could not be perceived even at small distances; solid walls exercise a powerful damping effect upon it. In the lecture-room referred to I ascertained the distribution of the force as follows:—Wherever the direction of the force could be definitely determined I marked it by a chalk line on the floor; but wherever it could not well be determined I drew a star upon the floor. Fig. 23 shows on a reduced scale a portion of the diagram thus made; with reference to it we note:—

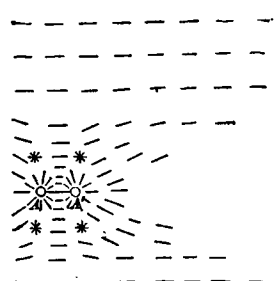


Fig. 23.

1. At distances beyond 3 metres the force is everywhere parallel to the primary oscillation. This is clearly the region in which the electrostatic force has become negligible, and the electromagnetic force alone is effective. All theories agree in this—that the electromagnetic force of a current-element is inversely proportional to the distance, whereas the electrostatic force (as the difference between the effects of the two poles) is inversely proportional to the third power of the distance.

It is worthy of notice that, in the direction of the oscillation, the action becomes weaker much more rapidly than in the perpendicular direction, so that in the former direction the effect can scarcely be perceived at a distance of 4 metres, whereas in the latter direction it extends at any rate farther than 12 metres. Many of the elementary laws of induction which are accepted as possible will have to be abandoned if tested by their accordance with the results of these experiments.

2. As already stated, at distances less than a metre the character of the distribution is determined by the electrostatic force.

3. Along one pair of straight lines the direction of the force can be determined at every point. The first of these straight lines is the direction of the primary oscillation itself; the second is perpendicular to the primary oscillation through its centre. Along the latter the magnitude of the force is at no point zero; the size of the sparks induced by it diminishes steadily from greater to smaller values. In this respect also the phenomena contradict certain of the possible elementary laws which require that the force should vanish at a certain distance.

4. One remarkable fact that results from the experiment is, that there exist regions in which the direction of the force cannot be determined; in our diagram each of these is indicated by a star. These regions form in space two rings around the rectilinear oscillation. The force here is of approximately the same strength in all directions, and yet it cannot act simultaneously in these different directions; hence it must assume in succession these different directions. Hence the phenomenon can scarcely be explained otherwise than as follows:—The force does not retain the same direction and alter its magnitude; its magnitude remains approximately constant, while its direction changes, passing during each oscillation round all the points of the compass. I have not succeeded in finding an explanation of this behaviour, either in the terms which have been neglected in our simplified theory, or in the harmonics which are very possibly mingled with our fundamental vibration. And it seems to me that none of the theories which are based upon the supposition of direct action-

at-a-distance would lead us to expect anything of this kind. But the phenomenon is easily explained if we admit that the electrostatic force and the electromagnetic force are propagated with different velocities. For in the regions referred to these two forces are perpendicular to one another, and are of the same order of magnitude; hence if an appreciable difference of phase has arisen between them during the course of their journey, their resultant—the total force—will, during each oscillation, move round all points of the compass without approaching zero in any position.

A difference between the rates of propagation of the electrostatic and electromagnetic forces implies a finite rate of propagation for at least one of them. Thus it seems to me that we probably have before us here the first indication of a finite rate of propagation of electrical actions.

In an earlier paper [1] I mentioned that trivial details, without any apparent reason, often interfered with the production of oscillations by the primary spark. One of these, at any rate, I have succeeded in tracing to its source. For I find that when the primary spark is illuminated, it loses its power of exciting rapid electric disturbances. Thus, if we watch the sparks induced in a secondary conductor, or in any auxiliary conductor attached to the discharging circuit, we see that these sparks vanish as soon as a piece of magnesium wire is lit, or an arc light started, in the neighbourhood of the primary spark. At the same time the primary spark loses its crackling sound. The spark is particularly sensitive to the light from a second discharge. Thus the oscillations always cease if we draw sparks from the opposing faces of the knobs by means of a small insulated conductor; and this even though these sparks may not be visible. In fact, if we only bring a fine point near the spark, or touch any part of the inner surfaces of the knobs with a rod of sealing-wax or glass, or a slip of mica, the nature of the spark is changed, and the oscillations cease. Some experiments made on this matter seem to me to prove (and further experiments will doubtless confirm this) that in these latter cases as well the effective

[1] See No. II., p. 29.

cause of the change is the light of a side-flash, which is scarcely visible to the eye.

These phenomena are clearly a special form of that action of light upon the electric discharge, of which one form was first described by myself some time ago, and which has since been studied in other forms by Herren E. Wiedemann, H. Ebert, and W. Hallwachs.

VI

ON ELECTROMAGNETIC EFFECTS PRODUCED BY ELECTRICAL DISTURBANCES IN INSULATORS

(*Sitzungsber. d. Berl. Akadem.* Nov. 10, 1887. *Wiedemann's Ann.* **34**, p. 273.)

IT is obviously a fundamental assumption in the most promising electrical theories that electrical disturbances in insulators are accompanied, not only by the electrostatic actions (which are known with certainty to exist), but also by the corresponding electromagnetic actions. All that we know about electrical phenomena has long tended to raise this assumption to a high degree of probability; but as yet it can scarcely be said to follow with certainty from any direct observations. In the following pages are described a series of researches which will, I hope, assist in filling this gap. They exhibit an electromagnetic effect which proceeds from insulators; they can be repeated with unfailing success and without extensive appliances. A magnetic effect arising out of processes in an insulator has already been exhibited in an experiment by Herr Röntgen,[1] if we assume that the final communication relating to this experiment confirms the interpretation first assigned to it.

In order to detect the electromagnetic action, I made use of the extremely rapid electric oscillations which can be excited in unclosed metallic conductors by the appropriate use of sparks.[2] The method is the following:—A primary conductor in which oscillations of the kind referred to are excited, acts inductively

[1] W. C. Röntgen, *Sitzungsber. d. Berl. Acad.*, 1885, p. 195. Cp. also the more recent paper, *Sitzungsber. der Berl. Acad.*, 1888, p. 23.

[2] See II. and V.

upon a secondary conductor. The induced disturbance is observed by inserting a spark-gap. In order to make the observation delicate both conductors are adjusted to the same period of oscillation. The secondary conductor is now brought as near to the primary as possible, but in such a position that the forces acting upon its various parts neutralise each other, so that it remains free from sparks. If the equilibrium is now upset by bringing other conductors near, sparking commences again; the system acts as a kind of induction-balance. But it is an induction-balance which has this peculiarity, that it also indicates a change when large insulating masses are brought near it. For the oscillations are so rapid that the quantities of electricity displaced in insulators by dielectric polarisation are of the same order of magnitude as those which are set in motion by conduction in metals.

The Apparatus

Fig. 24 shows the apparatus by means of which this principle was put into practice. Only the essential parts are shown; we have to imagine them as connected by a light wooden frame. $A\,A'$ is the primary conductor, consisting of two square brass plates 40 cm. in the side, which are connected by a copper wire $^1/_2$ cm. thick and 70 cm. long. In the middle of the latter a spark-gap $^3/_4$ cm. is inserted; the poles consist of well-polished brass knobs. If we now conduct to the latter the most powerful discharges of a large induction-coil, the plates A and A' are first electrified in opposite senses and then, at the instant when the spark passes, discharge into one another, thereby giving rise to the oscillations which are peculiar to the conductor $A\,A'$, having a period which may be estimated as the hundred-millionth part of a second. The discharge of the induction-coil which immediately follows has no more effect upon the phenomena which we are here considering than has the presence of the induction-apparatus and the wires leading to it. The secondary conductor B forms an exact circle of 35 cm. radius, and is made of copper wire 2 mm. thick; it contains at f a spark-gap the length of which can be varied by a fine screw from a few millimetres down to a few hundredths of a millimetre. A circle having the above dimensions

is in resonance with the primary conductor, and when it is placed in a suitable position secondary sparks 6-7 mm. long can be obtained. For the purpose of our experiment the circle is mounted so that it can rotate about an axis passing through its centre and perpendicular to its plane; when the circle is rotated thus its position is not altered, but the spark-gap rotates with it. The position of the axis is such that its direction lies in the plane of the plates A and A', and in fact coincides with the line $m\ n$ which is symmetrical with respect to them. If we add that the smallest distance between $A\ A'$ and B is 12 cm., the description of our apparatus is complete. The phenomena which we now observe by means of it are the following:—

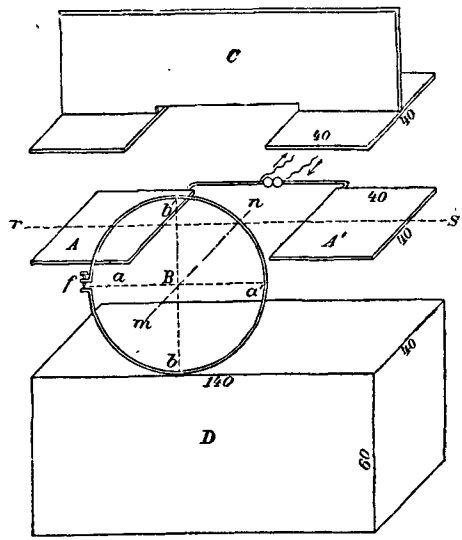


Fig. 24.

When the spark-gap f lies in the horizontal plane of $A\ A'$, *i.e.* at a or at a', it is entirely free from sparks. When the circle is rotated a few degrees in either direction from this position, minute sparks arise. These small sparks increase in length and strength as the spark-gap is removed farther from the position of equilibrium and reach a maximum length of about 3 mm. when f is at the highest and lowest points, b and b' respectively, of the circle. The oscillations of the secondary conductor which are thus made manifest are always due to the forces acting upon those parts of the circle B which are opposite to the spark-gap. Although in form it is nearly closed, B must be regarded as an unclosed circuit; those parts of it which lie on either side of the spark-gap act only as capacities of the ends of the current. The effective force is the resultant of the electrostatic force and the electromagnetic force which

is opposed to it; the former, being the greater of the two, determines the direction of the total force. If we regard the direction of this force and the amplitude of the oscillation excited by it as being positive when f is at the highest point of the circle, then we must regard the force and the amplitude as being negative, with reference to a fixed direction in the circle B, when f is at the lowest point. The amplitude changes sign as it passes through zero in the position of equilibrium.

It will assist us in what follows if we also consider here the phenomena which occur when we shift the circle B a little downwards, parallel to itself and without moving it out of its plane. When this is done the sparking distance increases at the highest point and diminishes at the lowest point; the points which are free from sparks—the null-points as we may call them—no longer lie on the horizontal line through the axis, but appear to be rotated downwards through a certain angle on either side. The slight displacement has changed the effect of the force of induction, although it has scarcely changed the effect of the electrostatic force, for the former, when integrated around the closed circle B, now gives an integral which is not zero; hence it gives rise to an oscillation the sign of whose amplitude is independent of the position of the spark-gap; and according to our convention this sign is positive. For the direction of the integrated force of induction is opposite to that of the electrostatic force in the upper half, but is the same as that of the electrostatic force in the lower half of the circle B, in which latter we regard the sign of the electrostatic force as being positive. Since the oscillation which is now superposed does not differ in phase from the former one, their amplitudes are simply added together. This explains the results observed.

The explanations of the phenomena which we have here given will be found more completely established in the preceding paper.[1]

Effects produced by Approach of Conductors

Hitherto it has been assumed that the conductors $A\,A'$ and B are set up in a large room as far away as possible from all objects which might disturb the action. Such an arrange-

[1] See V.

ment is necessary if we wish to secure an actual disappearance of the sparks at a and a'. For we soon observe that sparks are produced when conductors are brought near, *e.g.* when long metal rods are placed on the floor underneath the apparatus. A little attention shows that even the body of the observer exerts a perceptible influence. If he places himself 1-2 metres away on the prolongation of the axis $m\,n$ the apparatus is free from sparks; but if he approaches nearer in order to examine the sparks, he always finds them present. These very minute sparks have to be observed from a distance, and it follows as a necessary consequence that the observer must work in a dark room, and that his eyes must be rendered more sensitive by not exposing them to light beforehand.

We have now to choose a conductor which will produce a moderately large effect, and of which we may assume the oscillation period to be smaller than that of our primary oscillation. These conditions are fulfilled by the conductor made of sheet-metal, which is shown at C in our illustration. When it is lowered towards the primary conductor $A\,A'$, we observe the following effects:—The spark-length has decreased at the highest point b, and has increased at the lowest point b'; the null-points have moved upwards, *i.e.* towards the conductor C, whereas there now is noticeable sparking where the null-points originally were. From the last experiment in the preceding section we know what effect would be produced by shifting the conductor $A\,A'$ upwards. The same effect—qualitatively—would be produced by introducing above $A\,A'$ a second current having the same direction as that in $A\,A'$. Now our conductor C exerts exactly the opposite effect; and, if we assume that there exists in C a current which is always in the opposite direction to that in $A\,A'$, this effect is naturally explained as being due to an inductive action proceeding from C. This assumption is indeed necessary, for the preponderating electrostatic force tends to produce such a current; and, since the natural period of oscillation of the conductor is less than that of the force, the current must have the same phase as the exciting force. In order to test the correctness of this explanation I proceeded to experiment further in the following way:—I left the horizontal plates of the conductor C in position, but removed the vertical sheet, and in place of it

introduced in succession longer and thinner wires, with a view to increasing gradually the period of oscillation of the conductor C. The results of this progressive change were as follows:—At first the null-points retreated farther and farther upwards, but at the same time became more and more indistinct; they were no longer points of extinction, but simply points of minimum spark-length. Hitherto the spark-length at the highest point was much smaller than at the lowest point; but after the disappearance of the zero-points it began to increase again. At a certain stage the sparks in the highest and lowest positions again became equal, but no null-points could be found anywhere in the circle; in all positions there was vigorous sparking. From here on the spark-length at the lowest point grew less, and in its neighbourhood there presently appeared two null-points, which at first were only feebly marked; these soon became more distinct, and approached towards the points α and α', but always lay on the half of the circle remote from the conductor C. Finally they coincided with the points α and α'; the electrical condition was now identical with that which obtained before the conductor C had been brought near. The successive changes are just what might be expected according to our conception of the mode of action. For if the period of oscillation of the conductor C approaches that of the conductor $A\,A'$, the current in C becomes stronger, but at the same time there arises a difference of phase between the current and the inducing force. At the stage where resonance occurs the current in C is strongest, and the difference of phase amounts (as in every case of resonance of a moderately damped oscillation) to a quarter-period; hence there can no longer be any interference between the oscillations induced in B by $A\,A'$ and by C respectively. This condition evidently corresponds to the stage specially referred to above. If the period of oscillation of C becomes much greater than that of $A\,A'$ the amplitude of the oscillations in C again decreases, and the difference of phase between them and the exciting force now approaches a half-period. The current in C is now at every instant in the same direction as that in $A\,A'$; interference between the oscillations excited in B by

these currents is again possible; but the effect produced by the conductor C must be opposite to that which it exerted in its original position.

If the conductor C is brought very close to $A\,A'$ only small sparks appear in the circle B. By bringing the conductors closer together the period of oscillation of $A\,A'$ is increased, and thus $A\,A'$ and B are no longer in resonance.

Effects produced by Approach of Non-conductors

A very rough estimate shows that if large masses of insulating substances are brought near to the apparatus, the quantities of electricity displaced by dielectric polarisation must be at least as great as those which are set in motion by conduction in thin metallic rods. The approach of the latter has been found to produce a very noticeable effect in our apparatus; if, therefore, the approach of large insulating masses produced no similar effect, we should naturally conclude that the electricity displaced by dielectric polarisation did not exert a corresponding electromagnetic action. But if the views of Faraday and Maxwell are correct, we should expect that a noticeable effect would be produced, and, further, that the approach of a non-conductor would act in the same way as that of a conductor having a very short period of oscillation. Experiment fully confirms this expectation; and the only difficulty in carrying out the experiments is that of procuring sufficiently large masses of the insulating material.

I made the first experiments with a material which lay ready to hand, namely, paper. Underneath the conductor $A\,A'$ I piled up books in the form of a parallelopiped 1·5 metre long, 0·5 m. broad, and 1 m. high, until they reached the plates A and A'. It was shown without doubt that sparks now appeared in those positions of the circle which before were free from sparks, and that in order to make the sparks disappear the spark-gap f had to be turned about 10° towards the pile of books. Encouraged by this, I had 800 kgm. of unmixed asphalt cast in the form of a block 1·4 metre long, 0·6 m. high, and 0·4 m. broad (D in Fig. 24). The apparatus was removed on to this, the plates being laid upon the block. The effect could

52163

be immediately recognised; the results obtained were as follows:—

1. The spark at the highest point of the circle was now considerably stronger than at the lowest point (that nearest the asphalt).

2. The null-points were displaced downwards, *i.e.* towards the insulator, and when the plates were laid right upon it the angle of displacement (which could be measured with fair accuracy) was 23°. But the sparking no longer ceased completely at these points. At the original zero-points there was now vigorous sparking.

3. When the plates A and A' rested upon the asphalt block the period of oscillation of $A\,A'$ was altered; the period of oscillation of B had to be increased at the same time in order to obtain sparks of maximum length.

4. If the apparatus was gradually removed in any direction away from the asphalt block the effect continuously diminished, without experiencing any qualitative change.

We have here all the effects of a conductor of small period of oscillation. The accordance between the mode of action of the insulator and of a conductor is further shown by the fact that the one can be compensated by the opposing action of the other. Thus, if the apparatus lay upon the asphalt, and the conductor C was brought near it from above, the null-points shifted backwards towards their original positions, and they again coincided with the points a and a' when the conductor C' was brought within about 11 cm. of the conductor $A\,A'$. If the upper surface of the asphalt lay 5 cm. beneath the plates A and A', compensation was attained as soon as C was brought within 17 cm. of $A\,A'$. The compensating action always took place when the conductor was somewhat farther off than the insulator. In a rough way these experiments show that the action of the insulator is, quantitatively as well as qualitatively, about what we should expect.

The asphalt used was an excellent insulator; it contained (as might be suspected from its high specific gravity) a large amount of mineral matter. One hundred parts (by weight) were found to give no less than 62 parts of ash, consisting of 17 parts of quartz-sand, 40 of calcium compounds, and 5 of aluminium and

iron compounds.[1] It might be suspected that the action should be attributed entirely to these constituents, some of which might perhaps act as conductors. In order to remove this doubt I had a second, and exactly similar, block made of the so-called artificial pitch: this also is an excellent insulator, and gives scarcely any ash. The phenomena observed with this were the same as those above described, excepting that they were not quite so strongly marked; for example, the maximum displacement of the null-points here was only 19°. Unfortunately, however, this artificial pitch contains not only hydrocarbons but also free carbon in a very fine state of division, and it would be difficult to determine the amount of this latter. It cannot be denied that this carbon would have some conductivity, and hence the doubt in question is not entirely removed by this experiment. The expense of undertaking further investigations on the same large scale with pure substances was prohibitory. I therefore had the system of conductors A A' and B made again of exactly one-half the linear dimensions, and tried whether the phenomena could be followed with sufficient accuracy in this smaller model. The result was satisfactory, although, of course, with such exceedingly delicate sparks the strain upon the observer's attention was necessarily increased. For the purpose of demonstrating the phenomenon, or for quantitative experiments, it would be advisable to adhere to the larger dimensions. With the small apparatus I investigated altogether eight substances, which I will now mention in order:—

1. *Asphalt.*—The large block already described was used. When the plates A and A' lay upon the block, so that their front edges lay along the front edge of the block, the rotation of the null-point amounted to 31°. When the apparatus was drawn forward, so that the central line r s coincided with the front edge of the block, the rotation amounted to 20°.

2. *Artificial Pitch obtained from Coal.*—Here, again, the large block was used. The rotations in the two positions referred to in (1) amounted to 21° and 13° respectively.

3. *Paper.*—When the apparatus was placed upon a block of paper 70 cm. long, 35 cm. high, and 20 cm. broad, the null-points were displaced about 8° towards the paper.

[1] For the analysis I have to thank my colleague, Herr Hofrath Engler.

4. With a block of dense and perfectly dry wood the rotation of the null-points amounted to about 10°.

5. *Sandstone.*—When the apparatus was brought near to a sandstone pillar in the building (almost touching it), the null-points were rotated about 20° towards the sandstone. I had already observed with the large apparatus that the stone floor exercised a perceptible effect as soon as the apparatus was brought within half a metre of it.

6. *Sulphur.*—A massive block 70 cm. long, 20 cm. broad, and 35 cm. high, was cast from roll sulphur in a wooden mould, and the mould was then removed. The action of the block was very distinct; the various effects described above could be perceived, and the rotation amounted to 13-14°.

7. *Paraffin.*—The paraffin was white; it melted between 60° and 70°, giving a liquid as clear as water and free from impurities. It was melted and poured into a cardboard mould of the same dimensions, which was afterwards removed. The action was very distinct, and the rotation amounted to 7°.

8. *Petroleum.*—In order to investigate the effect of a liquid insulator, I filled an oak trough with 45 litres of pure petroleum. The internal dimensions of the trough were:—Length 70 cm., breadth 20 cm., depth 35 cm. When full it produced a rotation of about 7°, when empty about 2°. The very perceptible difference indicates the effect which would be produced by the petroleum alone.

The concordance between the observations made upon so many substances, some of which were pure, scarcely leaves any doubt that the action is a real one, and that it must be attributed to the substances themselves, and not to impurities in them. Indeed, I see only two objections which can be urged against this interpretation of the phenomena, and it will be advisable to rebut these at once. In the first place, it might be asserted that the effect is not an electromagnetic one, but that the insulator changes the distribution of the electrostatic force in its neighbourhood, and that this change in the distribution results in a change in the phenomenon. I have tried in vain to interpret, in accordance with this assumption, the various phenomena observed. But the assertion can be directly disproved. For, if the insulator fills a space which is

only bounded by lines of force, and by parts of the surfaces of A and A', it cannot give rise to any change in the electrostatic force outside its own mass. Now the vertical plane through the centre line $r\ s$ is certainly made up of lines of force, and so also is the horizontal plane of the plates A and A' themselves. If, therefore, the insulator is bounded by these two planes, and if it extends behind the former and under the latter, as far as it can exert any influence, then every electrostatic effect outside the insulator is avoided. Now if we place our smaller apparatus with the line $r\ s$ upon the upper front edge of one of the large blocks, the conditions referred to are sufficiently fulfilled. But when this was done the action, as already stated, did not cease, but was of similar strength to that observed under the most favourable conditions. It follows that the action did not arise from electrostatic forces.

In the second place, it may be objected that the effects should be attributed to currents arising through a residual conductivity. This objection can scarcely be urged with respect to such excellent insulators as sulphur and paraffin; nor do I believe that it is valid in the case of inferior insulators such as wood. Even assuming that such a substance insulates so badly that it allows the charged plate A to discharge in the ten-thousandth part of a second, but not much more rapidly, then during an oscillation of our apparatus the plate would never lose more than the ten-thousandth part of its charge. The conduction-current proper in the substance under consideration would therefore never exceed the ten-thousandth part of the primary current in $A\ A'$, and hence it would be quite ineffective. Hence in the case of the better insulators, at any rate, any assistance through conduction is excluded.

At present it does not appear to be possible to give any discussion of the quantitative relations of the experiments that would be of interest.

We have now seen what effect is produced upon the secondary circuit B by bringing a metallic conductor C near to the primary conductor $A\ A'$. If C was in resonance with

$A\ A'$, its action upon B could not interfere with the direct action of $A\ A'$. But at the same time, when the conditions for resonance were fulfilled, its action was fairly powerful, and could even be perceived when C was removed 1-1·5 metre away from $A\ A'$. Upon this I based experiments which should establish a finite rate of propagation of the electric forces. For if these forces require time to proceed in the first place from $A\ A'$ to C, and then again from C back to B, the difference of phase between the effects of $A\ A'$ upon B and of C upon B will increase when the distance between $A\ A'$ and C increases; and the two effects must again become capable of producing interference if the distance between $A\ A'$ and C becomes so great that the time taken by the electric force in traversing it is one-quarter of the half-period of oscillation. Hitherto these experiments have been unsuccessful, for I have not been able to detect any of the phenomena which I had expected. But since it was at best a question of observing exceedingly delicate changes, I do not consider that this negative result should weigh against the positive results which I have obtained otherwise.

VII

ON THE FINITE VELOCITY OF PROPAGATION OF ELECTROMAGNETIC ACTIONS

(*Sitzungsbr. d. Berl. Akad. d. Wiss.* Feb. 2, 1888. *Wiedemann's Ann.* **34**, p. 551.)

WHEN variable electric forces act within insulators whose dielectric constants differ appreciably from unity, the polarisations which correspond to these forces exert electromagnetic effects. But it is quite another question whether variable electric forces in air are also accompanied by polarisations capable of exerting electromagnetic effects. We may conclude that, if this question is to be answered in the affirmative, electromagnetic actions must be propagated with a finite velocity.

While I was vainly casting about for experiments which would give a direct answer to the question raised, it occurred to me that it might be possible to test the conclusion, even if the velocity under consideration was considerably greater than that of light. The investigation was arranged according to the following plan :—In the first place, regular progressive waves were to be produced in a straight, stretched wire by means of corresponding rapid oscillations of a primary conductor. Next, a secondary conductor was to be exposed simultaneously to the influence of the waves propagated through the wire and to the direct action of the primary conductor propagated through the air; and thus both actions were to be made to interfere. Finally, such interferences were to be produced at different distances from the primary circuit, so as to find out whether the oscillations of the electric

force at great distances would or would not exhibit a retardation of phase, as compared with the oscillations in the neighbourhood of the primary circuit. This plan has proved to be in all respects practicable. The experiments carried out in accordance with it have shown that the inductive action is undoubtedly propagated with a finite velocity. This velocity is greater than the velocity of propagation of electric waves in wires. According to the experiments made up to the present time, the ratio of these velocities is about 45 : 28. From this it follows that the absolute value of the first of these is of the same order as the velocity of light. Nothing can as yet be decided as to the propagation of electrostatic actions.

The Primary and Secondary Conductors

The primary conductor $A\,A'$ (Fig. 25) consisted of two square brass plates, 40 cm. in the side, which were connected

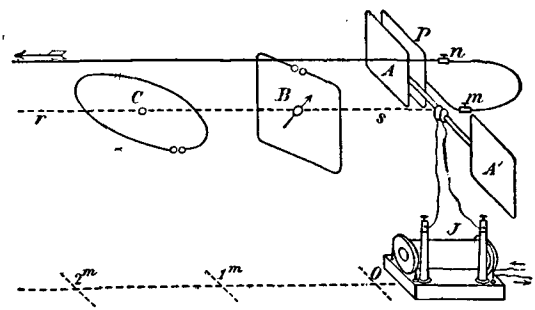


Fig. 25.

by a copper wire 60 cm. long. In the middle of the wire was a spark-gap in which oscillations were produced by very powerful discharges of an induction-coil *J*. The conductor was set up 1·5 metre above the floor, with the wire horizontal and the plane of the plates vertical. We shall denote as the base-line of our experiments a horizontal straight line *r s* passing through the spark-gap and perpendicular to the direction of the primary oscillation. We shall denote as the zero-point a point on this base-line 45 cm. from the spark-gap.

The experiments were carried out in a large lecture-room, in which there were no fixtures for a distance of 12 metres in

the neighbourhood of the base-line.[1] During the experiments this room was darkened.

The secondary circuit used was sometimes a wire C in the form of a circle of 35 cm. radius, sometimes a wire B bent into a square of 60 cm. in the side. The spark-gap of both these conductors was adjustable by means of a micrometer-screw; and in the case of the square conductor the spark-gap was provided with a lens. Both conductors were in resonance with the primary conductor. As calculated from the capacity and coefficient of self-induction of the primary, the (half) period of oscillation of all three conductors amounted to 1·4 hundred-millionths of a second.[2] Still it is doubtful whether the ordinary theory of electric oscillations gives correct results here. But inasmuch as it gives correct values in the case of Leyden jar discharges, we are justified in assuming that its results in the present case will, at any rate, be correct as far as the order of magnitude is concerned.

Let us now consider the influence of the primary oscillation upon the secondary circuit in some of the positions which are of importance in our present investigation. First let us place the secondary conductor with its centre on the base-line and its plane in the vertical plane through the base-line. We shall call this the first position. In this position no sparks are perceived in the secondary circuit. The reason is obvious: the electric force is at all points perpendicular to the direction of the secondary wire.

Now, leaving the centre of the secondary conductor still on the base-line, let it be turned so that its plane is perpendicular to the base-line; we shall call this the second position. Sparks now appear in the secondary circuit whenever the spark-gap lies above or below the horizontal plane through the base-line; but no sparks appear when the spark-gap lies in this plane. As the distance from the primary oscillator increases, the length of the sparks diminishes, at first rapidly but afterwards very slowly. I was able to observe the sparks along the whole distance (12 metres) at my disposal, and have no doubt that in larger rooms this distance could be still farther extended. In this position the sparks owe their origin

[1] [See Note 12 at end of book.]
[2] See II., p. 50. [See also Note 13 at end of book.]

mainly to the electric force which always acts in the part of the secondary circuit opposite to the spark-gap. The total force may be split up into the electrostatic part and the electromagnetic part; there is no doubt that at short distances the former, at greater distances the latter, preponderates and settles the direction of the total force.

Finally, let the plane of the secondary conductor be brought into the horizontal position, its centre being still on the base-line. We shall call this the third position. If we use the circular conductor, place it with its centre at the zero-point of the base-line, and turn it so that the spark-gap slowly moves around it, we observe the following effects:— In all positions of the spark-gap there is vigorous sparking. The sparks are most powerful and about 6 mm. long when the spark-gap faces the primary conductor; they steadily diminish when the spark-gap is moved away from this position, and attain a minimum value of about 3 mm. on the side farthest from the primary conductor. If the conductor was exposed only to the electrostatic force, we should expect sparking when the spark-gap was on the one side or the other in the neighbourhood of the base-line, but no sparking in the two intermediate positions. Indeed, the direction of the oscillation would be determined by the direction of the force in the portion of the secondary conductor lying opposite to the spark-gap. But upon the oscillation excited by the electrostatic force is superposed the oscillation excited by the electromagnetic force; and here the latter is very powerful, because the electromagnetic force when integrated around the secondary circuit (considered as being closed) gives a finite integral value. The direction of this integrated force of induction is independent of the position of the spark-gap; it opposes the electrostatic force in the part of the secondary conductor which faces $A\,A'$, but reinforces the electrostatic force in the part which faces away from $A\,A'$. Hence the electrostatic and electromagnetic forces assist each other when the spark-gap is turned towards, but they oppose each other when it is turned away from the primary conductor. That it is the electromagnetic force which preponderates in the latter position and determines the direction of the oscillation, may be recognised from the fact that the change from the one state

to the other takes place in any position without any extinction of the sparks. For our purpose it is important to make the following observations:—If the spark-gap is rotated to the right or left through 90° from the base-line, it lies at a nodal point with respect to the electrostatic force, and the sparks which appear in it owe their origin entirely to the electromagnetic force, and especially to the fact that the latter, around the closed circuit, is not zero. Hence, in this particular position, we can investigate the electromagnetic effect, even in the neighbourhood of the primary conductor, independently of the electrostatic effect.

A complete demonstration of the above explanations will be found in an earlier paper.[1] Some further evidence in support of these explanations, and of the results arrived at in my earlier paper, will be found in what follows.

The Waves in the Straight Wire

In order to excite in a wire with the aid of our primary oscillations waves suitable for our purpose, we proceed as follows:—Behind the plate A we place a plate P of the same size. From the latter we carry a copper wire 1 mm. thick to the point m on the base-line; from there, in a curve 1 metre long, to the point n, which lies 30 cm. above the spark-gap, and thence in a straight line parallel to the base-line for a distance sufficiently great to prevent any fear of disturbance through reflected waves. In my experiments the wire passed through the window, then went about 60 metres freely through the air, and ended in an earth-connection. Special experiments showed that this distance was sufficiently great. If now we bring near to this wire a metallic conductor in the form of a nearly closed circle, we find that the discharges of the induction-coil are accompanied by play of small sparks in the circle. The intensity of the sparks can be altered by altering the distance between the plates P and A. That the waves in the wire have the same periodic time as the primary oscillations, can be shown by bringing near to the wire one of our tuned secondary conductors; for in these the sparks become more powerful than in any other metallic circuits, whether

[1] See V., p. 80.

larger or smaller. That the waves are regular, in respect to space as well as time, can be shown by the formation of stationary waves. In order to produce these, we allow the wire to end freely at some distance from its origin, and bring near to it our secondary conductor in such a position that its plane includes the wire, and that the spark-gap is turned towards the wire. We then observe that at the free end of the wire the sparks in the secondary conductor are very small; they increase in length as we move towards the origin of the wire; at a certain distance, however, they again decrease and sink nearly to zero, after which they again become longer. We have thus found a nodal point. If we now measure the wave-length so found, make the whole length of the wire (reckoned from the point n) equal to a complete multiple of this length, and repeat the experiment, we find that the whole length is now divided up by nodal points into separate waves.[1] If we fix each nodal point separately with all possible care, and indicate its position by means of a paper rider, we see that the distances of these are approximately equal, and that the experiments admit of a fair degree of accuracy.

The nodes can also be distinguished from the antinodes in other ways. If we bring the secondary conductor near to the wire, in such a position that the plane of the former is perpendicular to the latter, and that the spark-gap is neither turned quite towards the wire nor quite away from it, but is in an intermediate position, then our secondary circle is in a suitable position for indicating the existence of forces which are perpendicular to the direction of the wire. Now, when the circle is in such a position, we see that sparks appear at the nodal points, but disappear at the antinodes. If we draw sparks from the wire by means of an insulated conductor, we find that these are somewhat stronger at the nodes than at the antinodes; but the difference is slight, and for the most part can only be perceived when we already know where the nodes and antinodes respectively are situated. The reason why this latter method and other similar ones give no definite result is that the particular waves under consideration have other irregular disturbances superposed upon them. With the aid of our tuned circle, however, we can pick out the disturbances in

[1] [See Note 14 at end of book.]

which we are interested, just as particular notes can be picked out of confused noises by means of resonators.

If we cut through the wire at a node, the phenomena along the part between it and the origin are not affected: the waves are even propagated along the part which has been cut off if it is left in its original position, although their strength is diminished.

The fact that the waves can be measured admits of numerous applications. If we replace the copper wire hitherto used by a thicker or thinner copper wire, or by a wire of another metal, the nodal points are found to remain in the same positions. Thus the rate of propagation in all such wires is the same, and we are justified in speaking of it as a definite velocity. Even iron wires are no exception to this general rule; hence the magnetic properties of iron are not called into play by such rapid disturbances. It will be of interest to test the behaviour of electrolytes. The fact that the electrical disturbance in these is bound up with the disturbance of inert matter might lead us to expect a smaller velocity of propagation.[1] Through a tube of 10 mm. diameter, filled with a solution of copper sulphate, the waves would not travel at all; but this may have been due to the resistance being too great. Again, by measuring the wave-lengths, we can determine the relative periods of oscillation of different primary conductors; it should be possible to compare in this way the periods of oscillation of plates, spheres, ellipsoids, etc.

In our particular case the nodal points proved to be very distinct when the wire was cut off at a distance of either 8 metres or 5·5 metres from the zero-point of the base-line. In the former case the positions of the paper riders used for fixing the nodal points were—0·2 m., 2·3 m., 5·1 m., and 8 m.; in the latter case—0·1 m., 2·8 m., and 5·5 m., the distances being measured from the zero-point. From this it appears that the (half) wave-length in the free wire cannot differ much from 2·8 metres. We can scarcely be surprised at finding that the first wave-length, reckoned from P, appears smaller than the rest, when we take into consideration the presence of the plate and the bending of the wire. A period of oscilla-

[1] [See Note 15 at end of book.]

tion of 1·4 hundred-millionths of a second, and a wave-length of 2·8 metres, gives 200,000 km./sec. as the velocity of electric waves in wires.[1] In the year 1850 Fizeau and Gounelle,[2] making use of a very good method, found for this velocity the value 100,000 km./sec. in iron wires, and 180,000 km./sec. in copper wires. In 1875 W. Siemens,[3] using discharges from Leyden jars, found velocities from 200,000 to 260,000 km./sec. in iron wires. Other determinations can scarcely be taken into consideration. Our result comes in well between the above experimental values. Since it was obtained with the aid of a doubtful theory, we are not justified in publishing it as a new measurement of this same velocity; but, on the other hand, we may conclude, from the accordance between the experimental results, that our calculated value of the period of oscillation is of the right order of magnitude.

Interference between the direct Action and that propagated through the Wire

Let us place the square circuit B at the zero-point in our second position, and so that the spark-gap is at the highest point. The waves in the wire now exert no influence; the direct action gives rise to sparks 2 mm. long. If we now bring B into the first position by turning it about a vertical axis, it is found conversely that the primary oscillation exercises no direct effect; but the waves in the wire now induce sparks which can be made as long as 2 mm. by bringing P near to A. In intermediate positions both causes give rise to sparks, and it is thus possible for them, according to their difference in phase, either to reinforce or to weaken each other. Such a phenomenon, in fact, we observe. For, if we adjust the plane of B so that its normal towards $A\,A'$ points away from that side of the primary conductor on which the plate P is placed, the sparking is even stronger than it is in the principal positions; but if we adjust the plane of B so that its normal points towards P, the sparks disappear, and only reappear

[1] [See Note 16 at end of book.]
[2] Fizeau and Gounelle, *Pogg. Ann.* **80**, p. 158, 1850.
[3] W. Siemens, *Pogg. Ann.* **157**, p. 309, 1876.

when the spark-gap has been considerably shortened. If, under the same conditions, we place the spark-gap at the lowest point of B, the disappearance of the sparks takes place when the normal points away from P. Further modifications of the experiment—*e.g.* by carrying the wire beneath the secondary conductor—produce just such effects as might be expected from what has above been stated. The phenomenon itself is just what we expected; let us endeavour to make it clear that the action takes place in the sense indicated in our explanation. In order to fix our ideas, let us suppose that the spark-gap is at the highest point, and the normal turned towards P (as in the figure). At the particular instant under consideration let the plate P have its largest positive charge. The electrostatic force, and therefore the total force, is directed from A towards A'. The oscillation induced in B is determined by the direction of the force in the lower part of B. Positive electricity will therefore be urged towards A' in the lower part, and away from A' in the upper part. Let us now consider the action of the waves. As long as A is positively charged, positive electricity flows away from the plate P. At the instant under consideration this flow reaches its maximum development in the middle of the first half wave-length of the wire. At a quarter wave-length farther from the origin—that is, in the neighbourhood of our zero-point—it is just beginning to take up this direction (away from the zero-point). Hence at this point the electromagnetic induction urges positive electricity in its neighbourhood towards the origin. In particular, positive electricity in our conductor B is thrown into a state of motion in a circle, so that in the upper part it tends to flow towards A', and in the lower part away from A'. Thus, in fact, the electrostatic and electromagnetic forces act against one another, and are in approximately the same phase; hence they must more or less annul one another. If we rotate the secondary circle through 90° (through the first position) the direct action changes its sign, but the action of the waves does not; both causes reinforce one another. The same holds good if the conductor B is rotated in its own plane until the spark-gap lies at its lowest point.

We now replace the wire $m\ n$ by longer lengths of wire. We observe that this renders the interference more indistinct;

it disappears completely when a piece of wire 250 cm. long is introduced; the sparks are of the same length whether the normal points away from P or towards it. If we lengthen the wire still more the difference of behaviour in the various quadrants again exhibits itself, and the extinction of the sparks becomes fairly sharp when 400 cm. of wire is introduced. But there is now this difference—that extinction occurs when the spark-gap is at the top, and the normal points away from P. Further lengthening of the wire causes the interference to disappear once more; but it reappears in the original sense when about 6 metres of wire are introduced. These phenomena are obviously explained by the retardation of the waves in the wire, and they also make it certain that the state of affairs in the progressive waves changes sign about every 2·8 metres.

If we wish to produce interference while the secondary circle C lies in the third position, we must remove the rectilinear wire from the position in which it has hitherto remained, and carry it along in the horizontal plane through C, either on the side towards the plate A, or on the side towards the plate A'. In practice it is sufficient to stretch the wire loosely, grasp it with insulating tongs, and bring it alternately near one side or the other of C. What we observe is as follows:—If the waves are carried along the side on which the plate P lies, they annul the sparks which were previously present; if they are carried along the opposite side they strengthen the sparks which were already present. Both results always occur, whatever may be the position of the spark-gap in the circle. We have seen that at the instant when the plate A has its strongest positive charge, and when, therefore, the primary current begins to flow away from A, the surging at the first nodal point of the rectilinear wire begins to flow away from the origin of the wire. Hence both currents flow round C in the same sense when the rectilinear wire lies on the side of C which is remote from A; in the other case they flow round C in opposite senses, and their actions annul one another. The fact that the position of the spark-gap is of no importance confirms our supposition that the direction of the oscillation is here determined by the electromagnetic force. The interferences which have just been described also

change their sign when 400 cm. of wire, instead of 100 cm., is introduced between the points *m* and *n*.

I have also produced interferences in positions in which the centre of the secondary circle lay outside the base-line; but for our present purpose these are only of importance inasmuch as they throughout confirmed our fundamental views.

Interference at Various Distances

Interferences can be produced at greater distances in the same way as at the zero-point. In order that they may be distinct, care must be taken that the action of the waves in the wire is in all cases of about the same magnitude as the direct action. This can be secured by increasing the distance between *P* and *A*. Now very little consideration will show that, if the action is propagated through the air with infinite velocity, it must interfere with the waves in the wire in opposite senses at distances of half a wave-length (*i.e.* 2·8 metres) along the wire. Again, if the action is propagated through the air with the same velocity as that of the waves in the wire, the two will interfere in the same way at all distances. Lastly, if the action is propagated through the air with a velocity which is finite, but different from that of the waves in the wire, the nature of the interference will alternate, but at distances which are farther than 2·8 metres apart.

In order to find out what actually took place, I first made use of interferences of the kind which were observed in passing from the first into the second position. The spark-gap was at the top. At first I limited myself to distances up to 8 metres from the zero-point. At the end of each half-metre along this position the secondary conductor was set up and examined in order to see whether any difference could be observed at the spark-gap according as the normal pointed towards *P* or away from it. If there was no such difference, the result of the experiment was indicated by the symbol O. If the sparks were smaller when the normal pointed towards *P*, then this showed an interference which was represented by the symbol +. The symbol − was used to indicate an inter-

ference when the normal pointed towards the other side. In order to multiply the experiments I frequently repeated them, making the wire *m n* 50 cm. longer each time, and thus lengthening it gradually from 100 cm. to 600 cm. The results of my experiments are contained in the following summary which will easily be understood:—

	0		1		2		3		4		5		6		7		8
100	+	+	0	−	−	−	−	−	0	0	0	0	0	+	+	+	+
150	+	0	−	−	−	−	0	0	0	0	0	+	+	+	+	+	0
200	0	−	−	−	−	−	0	+	+	+	+	+	0	0	0	0	0
250	0	−	−	−	−	0	0	+	+	+	+	0	0	0	0	0	0
300	−	−	−	−	0	+	+	+	+	+	0	0	0	0	−	−	−
350	−	−	0	+	+	+	+	+	+	0	0	0	−	−	−	−	−
400	−	−	0	+	+	+	+	0	0	0	0	−	−	−	−	−	−
450	−	0	+	+	+	+	+	0	0	0	−	−	−	−	−	−	0
500	−	0	+	+	+	+	0	−	−	−	−	−	0	0	0	0	+
550	0	+	+	+	+	0	0	−	−	−	−	−	0	0	0	0	+
600	+	+	+	+	0	0	−	−	−	−	−	0	0	+	+	+	+

According to this it might almost appear as if the interferences changed sign at every half wave-length of the waves in the wire.[1] But, in the first place, we notice that this does not exactly happen. If it did, then the symbol O should recur at the distances 1 m., 3·8 m., 6·6 m., whereas it obviously recurs less frequently. In the second place, we notice that the retardation of phase proceeds more rapidly in the neighbourhood of the origin than at a distance from it. All the rows agree in showing this. An alteration in the rate of propagation is not probable. We can with much better reason attribute this phenomenon to the fact that we are making use of the total force (*Gesammtkraft*), which can be split up into the electrostatic force and the electromagnetic. Now, according to theory, it is probable that the former, which preponderates in the neighbourhood of the primary oscillation, is propagated more rapidly than the latter, which is almost the only factor of importance at a distance. In order first to settle what actually happens at a greater distance, I have extended the experiments to a distance of 12 metres, for at any

[1] [See Note 17 at end of book.]

rate three values of the length $m\,n$. I must admit that this required rather an effort. Here are the results:—

	0	1	2	3	4	5	6	7	8	9	10	11	12
100	+	0	−	−	0	0	0	+	+	+	+	+	0
250	0	−	−	0	+	+	0	0	0	0	−	−	−
400	−	0	+	+	0	0	−	−	−	−	0	0	0

If we assume that at considerable distances the electromagnetic action alone is effective, then we should conclude from these observations that the interference of this action with the waves in the wires only changes its sign every 7 metres.

In order now to investigate the electromagnetic force in the neighbourhood of the primary oscillation (where the phenomena are more distinct) as well, I made use of the interferences which occur in the third position when the spark-gap is rotated 90° away from the base-line. The sense of the interference at the zero-point has already been stated, and this sense will be indicated by the symbol −, whereas the symbol + will be used to denote an interference by conducting the waves past the side of C which is remote from P. This choice of the symbols will be in accord with the way in which we have hitherto used them. For since the electromagnetic force is opposed to the total force at the zero-point, our first table would also begin with the symbol −, provided that the influence of the electrostatic force could have been eliminated. Now experiment shows, in the first place, that interference still takes place up to a distance of 3 metres, and that it is of the same sign as at the zero-point. This experiment, repeated often and never with an ambiguous result, is sufficient to prove the finite rate of propagation of the electromagnetic action. Unfortunately the experiments could not be extended to a greater distance than 4 metres, on account of the feeble nature of the sparks. Here, again, I repeated the experiments with variable lengths of the wire $m\ n$, so as to be able to verify the retardation of phase along this portion of the wire. The results are given in the following summary:—

	0	1	2	3	4		0	1	2	3	4
100	−	−	−	−	0	400	+	+	+	+	0
150	−	−	0	0	0	450	+	+	+	0	0
200	0	0	0	+	+	500	+	+	0	0	0
250	0	+	+	+	+	550	+	0	0	0	−
300	+	+	+	+	+	600	0	−	−	−	−
350	+	+	+	+	0						

A discussion of these results shows that here, again, the phase of the interference alters as the distance increases, so that a reversal of sign might be expected at a distance of 7-8 metres.

But this result is much more plainly shown by combining the results of the second and third summary—using the data of the latter up to a distance of 4 metres, and of the former for greater distances. In the first of these intervals we thus avoid the action of the electrostatic force by reason of the peculiar position of our secondary conductor; in the second this action drops out of account, owing to the rapid weakening of that force. We should expect the observations of both intervals to fit into one another without any break, and our expectation is confirmed. We thus obtain by collating the symbols the following table for the interference of the electromagnetic force with the action of the waves in the wire:—

	0	1	2	3	4	5	6	7	8	9	10	11	12
100	−	−	−	−	0	0	0	+	+	+	+	+	0
250	0	+	+	+	+	+	0	0	0	0	−	−	−
400	+	+	+	+	0	0	−	−	−	−	0	0	0

From this table I draw the following conclusions:—

1. The interference does not change sign every 2·8 metres. Therefore the electromagnetic actions are not propagated with infinite velocity.

2. The interference, however, is not in the same phase at all points. Therefore the electromagnetic actions do not spread out in air with the same velocity as the electric waves in wires.

3. A gradual retardation of the waves in the wire has the

effect of shifting any particular phase of the interference towards the origin of the waves. From the direction of this shifting it follows that of the two different rates of propagation that through air is the more rapid. For if by retardation of one of the two actions we bring about an earlier coincidence of both, then we must have retarded the slower one.

4. At distances of every 7·5 metres the sign of the interference changes from + to −. Hence, after proceeding every 7·5 metres, the electromagnetic action outruns each time a wave in the wire. While the former travelled 7·5 metres, the latter travelled $7{\cdot}5 - 2{\cdot}8 = 4{\cdot}7$ metres. The ratio of the two velocities is therefore as 75 : 47, and the half wave-length of the electromagnetic action in air is $2{\cdot}8 \times 75/47 = 4{\cdot}5$ metres. Since this distance is traversed in 1·4 hundred-millionths of a second, it follows that the absolute velocity of propagation in air is 320,000 km. per second. This result only holds good as far as the order of magnitude is concerned; still the actual value can scarcely be greater than half as much again, and can scarcely be less than two-thirds of the value stated. The actual value can only be determined by experiment when we are able to determine the velocity of electricity in wires more accurately than has hitherto been the case.

Since the interferences undoubtedly change sign after 2·8 metres in the neighbourhood of the primary oscillation, we might conclude that the electrostatic force which here predominates is propagated with infinite velocity. But this conclusion would in the main depend upon a single change of phase, and this one change can be explained (apart from any retardation of phase) by the fact that, at some distance from the primary oscillation, the amplitude of the total force undergoes a change of sign. If the absolute velocity of the electrostatic force remains for the present unknown, there may yet be adduced definite reasons for believing that the electrostatic and electromagnetic forces possess different velocities. The first reason is that the total force does not vanish at any point along the base-line. Since the electrostatic force preponderates at small distances, and the electromagnetic force at greater distances, they must in some intermediate position be equal and opposite, and, inasmuch as they do not annul one another, they must reach this position at different times.

The second reason is derived from the propagation of the force throughout the whole surrounding space. In a previous paper[1] it has already been shown how the direction of the force at any point whatever can be determined. The distribution of the force was there described, and it was remarked that there were four points in the horizontal plane, about 1·2 metre before and behind the outer edges of our plates A and A', at which no definite direction could be assigned to the force, but that the force here acts with about the same strength in all directions. The only apparent interpretation of this is that the electrostatic and electromagnetic components here meet one another at right angles, and are about equal in strength but differ notably in phase; thus they do not combine to produce a resultant rectilinear oscillation, but a resultant which during each oscillation passes through all points of the compass.

The fact that different components of the total force possess different velocities is also of importance, inasmuch as it provides a proof (independent of those previously mentioned) that at least one of these components must be propagated with finite velocity.

Conclusions

More or less important improvements in the quantitative results of this first experiment may result from further experiments in the same direction; but the path which they must follow may be said to be already made, and we may now regard it as having been proved that the inductive action is propagated with finite velocity. Sundry conclusions follow from the results thus obtained, and to some of these I wish to draw attention.

1. The most direct conclusion is the confirmation of Faraday's view, according to which the electric forces are polarisations existing independently in space. For in the phenomena which we have investigated such forces persist in space even after the causes which have given rise to them have disappeared. Hence these forces are not simply parts or attributes of their causes, but they correspond to changed con-

[1] See V., p. 80.

ditions of space. The mathematical character of these conditions justifies us then in denoting them as polarisations, whatever the nature of these polarisations may be.

2. It is certainly remarkable that the proof of a finite rate of propagation should have been first brought forward in the case of a force which diminishes in inverse proportion to the distance, and not to the square of the distance. But it is worth while pointing out that this proof must also affect such forces as are inversely proportional to the square of the distance. For we know that the ponderomotive attraction between currents and their magnetic actions are connected by the principle of the conservation of energy with their inductive actions in the strictest way, the relation being apparently that of action and reaction. If this relation is not merely a deceptive semblance, it is not easy to understand how the one action can be propagated with a finite and the other with an infinite velocity.

3. There are already many reasons for believing that the transversal waves of light are electromagnetic waves; a firm foundation for this hypothesis is furnished by showing the actual existence in free space of electromagnetic transversal waves which are propagated with a velocity akin to that of light. And a method presents itself by which this important view may finally be confirmed or disproved. For it now appears to be possible to study experimentally the properties of electromagnetic transversal waves, and to compare these with the properties of light waves.

4. The hitherto undecided questions of electromagnetics which relate to unclosed currents should now be more easily attacked and solved. Some of these questions, indeed, are directly settled by the results which have already been obtained. In so far as electromagnetics only lacks certain constants, these results might even suffice to decide between the various conflicting theories, assuming that at least one of them is correct.

Nevertheless, I do not at present propose to go into these applications, for I wish first to await the outcome of further experiments which are evidently suggested in great number by our method.

VIII

ON ELECTROMAGNETIC WAVES IN AIR AND THEIR REFLECTION

(*Wiedemann's Ann.* 34, p. 610, 1888.)

I HAVE recently endeavoured to prove by experiment that electromagnetic actions are propagated through air with finite velocity.[1] The inferences upon which that proof rested appear to me to be perfectly valid; but they are deduced in a complicated manner from complicated facts, and perhaps for this reason will not quite carry conviction to any one who is not already prepossessed in favour of the views therein adopted. In this respect the demonstration there given may be fitly supplemented by a consideration of the phenomena now to be described, for these exhibit the propagation of induction through the air by wave-motion in a visible and almost tangible form. These new phenomena also admit of a direct measurement of the wave-length in air. The fact that the wave-lengths thus obtained by direct measurement only differ slightly from the previous indirect determinations (using the same apparatus), may be regarded as an indication that the earlier demonstration was in the main correct.

In experimenting upon the action between a rectilinear oscillation and a secondary conductor I had often observed phenomena which seemed to point to a reflection of the induction action from the walls of the building. For example, feeble sparks frequently appeared when the secondary conductor was so situated that any direct action was quite impossible, as was evident from simple geometrical considera-

[1] See VII., p. 107.

tions of symmetry; and this most frequently occurred in the neighbourhood of solid walls. In especial, I continually encountered the following phenomenon:—In examining the sparks in the secondary conductor at great distances from the primary conductor, when the sparks were already exceedingly feeble, I observed that in most positions of the secondary conductor the sparks became appreciably stronger when I approached a solid wall, but again disappeared almost suddenly close to the wall. It seemed to me that the simplest way of explaining this was to assume that the electromagnetic action, spreading outwards in the form of waves, was reflected from the walls, and that the reflected waves reinforced the advancing waves at certain distances, and weakened them at other distances, stationary waves in air being produced by the interference of the two systems. As I made the conditions more and more favourable for reflection the phenomenon appeared more and more distinct, and the explanation of it given above more probable. But without dwelling upon these preliminary trials I proceed at once to describe the principal experiments.

The physics lecture-room in which these experiments were carried out is about 15 metres long, 14 metres broad, and 6 metres high. Parallel to the two longer walls there are two rows of iron pillars, each of which rows behaves much like a solid wall towards the electromagnetic action, so that the parts of the room which lie outside these cannot be taken into consideration. Thus only the central space, 15 metres long, 8·5 metres broad, and 6 metres high, remained for the purpose of experiment. From this space I had the hanging parts of the gas-pipes and the chandeliers removed, so that it contained nothing except wooden tables and benches which could not well be removed. No objectionable effects were to be feared from these, and none were observed. The front wall of the room, from which the reflection was to take place, was a massive sandstone wall in which were two doorways, and a good many gas-pipes extended into it. In order to give the wall more of the nature of a conducting surface a sheet of zinc 4 metres high and 2 metres broad was fastened on to it; this was connected by wires with the gas-pipes and with a neighbouring water-pipe, and especial care was taken that any

electricity that might accumulate at the upper and lower ends of the sheet should be able to flow away as freely as possible.

The primary conductor was set up opposite the middle of this wall at a distance of 13 metres from it, and was therefore 2 metres away from the opposite wall. It was the same conductor that had already been used in the experiments on the rate of propagation. The direction of the conducting wire was now vertical; hence the forces which have here to be considered oscillate up and down in a vertical direction. The middle point of the primary conductor was 2·5 metres above the level floor; the observations were also carried out at the same distance above the floor, a gangway for the observer being built up with tables and boards at a suitable height. We shall denote as the normal a straight line drawn from the centre of the primary conductor perpendicularly to the reflecting surface. Our experiments are restricted to the neighbourhood of this normal; experiments at greater angles of incidence would be complicated by having to take into consideration the varying polarisation of the waves. Any vertical plane parallel to the normal will be called a plane of oscillation, and any plane perpendicular to the normal will be called a wave-plane.

The secondary conductor was the circle of 35 cm. radius, which had also been used before. It was mounted so as to revolve about an axis passing through its centre and perpendicular to its plane. In the experiments the axis was horizontal; it was mounted in a wooden frame, so that both circle and axis could be rotated about a vertical axis. For the most part it does well enough for the observer to hold the circle, mounted in an insulating wooden frame, in his hand, and then to bring it as may be most convenient into the various positions. But, inasmuch as the body of the observer always exercises a slight influence, the observations thus obtained must be controlled by others obtained from greater distances. The sparks too are strong enough to be seen in the dark several metres off; but in a well-lit room practically nothing can be seen, even at close quarters, of the phenomena which are about to be described.

After we have made these preparations the most striking phenomenon that we encounter is the following:—We place

the secondary circle with its centre on the normal and its plane in the plane of oscillation, and turn the spark-gap first towards the wall and then away from it. Generally the sparks differ greatly in the two positions. If the experiment is arranged at a distance of about 0·8 metre from the wall the sparks are much stronger when the spark-gap is turned towards the wall. The length of the sparks can be so regulated that a continuous stream of sparks passes over when the spark-gap is turned towards the wall, whereas no sparks whatever pass over in the opposite position. If we repeat the experiment at a distance of 3 metres from the wall we find, on the contrary, a continuous stream of sparks when the spark-gap is turned away from the wall, whereas the sparks disappear when the spark-gap is turned towards the wall. If we proceed further to a distance of 5·5 metres, a fresh reversal has taken place; the sparks on the side towards the wall are stronger than the sparks on the opposite side. Finally, at a distance of 8 metres from the wall, we find that another reversal has been executed; the sparking is stronger on the side remote from the wall, but the difference is no longer so noticeable. Nor does any further reversal occur; for it is prevented by the preponderating strength of the direct action and by the complicated forces which exist in the neighbourhood of the primary oscillation. Our figure (the scale in which indicates the distances from the wall) shows at *I.*, *II.*, *III.*, *IV.*, the secondary circle in those positions in which the sparks were most strongly developed. The alternating character of the conditions of the space is clearly exhibited.

At distances lying between those mentioned both sets of sparks under consideration were of equal strength, and in the immediate neighbourhood of the wall too the distinction between them diminishes. We may therefore denote these points—namely, the points *A*, *B*, *C*, *D* in the figure—as being nodal points in a certain sense. Still we must not consider the distance between any one of these points and the next as being the half wave-length. For if *all* the electrical disturbances change their direction in passing through one of these points, then the phenomena in the secondary circle should repeat themselves without reversal; for in the spark-length there is nothing which corresponds to a change of direction in the

oscillation. We should rather conclude from these experiments that in passing through any one of these points one part of the action undergoes reversal, while another part does not. On the other hand, it is allowable to assume that double the distance between any two of the points corresponds to the half wave-length, so that these points each indicate the end of a quarter wave-length. And, indeed, on the basis of this assumption and of the fundamental view just expressed, we shall arrive at a complete explanation of the phenomenon.

For let us suppose that a vertical wave of electric force proceeds towards the wall, is reflected with slightly diminished intensity, and so gives rise to stationary waves. If the wall

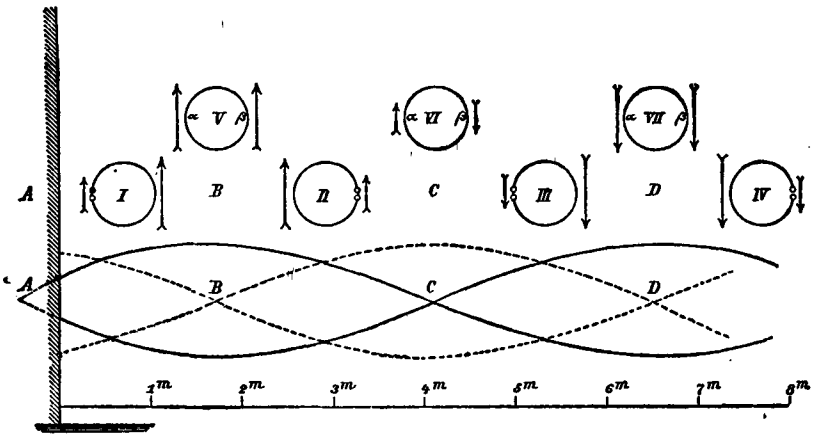


Fig. 26.

were a perfect conductor a node would form at its very surface. For inside a conductor or at its boundary the electric force must always be vanishingly small. Now our wall cannot be regarded as a perfect conductor. For, in the first place, it is only metallic in part, and the part which is metallic is not very extensive. Hence at its surface the force will still have a certain value, and this in the sense of the advancing wave. The node, which would be formed at the wall itself if it were perfectly conducting, must therefore lie really somewhat behind the surface of the wall, say at the point A in the figure. If double the distance $A\ B$, that is the distance $A\ C$, corresponds to the half wave-length, then the geometrical relations of the stationary wave are of the kind

which are represented in the usual symbolic fashion by the continuous wave-line in the figure. The forces acting on both sides of the circle in the positions *I, II, III,* and *IV* are correctly represented for any given instant in magnitude and direction by the arrows at the sides. If, then, in the neighbourhood of a node the spark-gap is turned towards the node, we have in the circle a stronger force acting under favourable conditions against a weaker force, which acts under unfavourable conditions. But if the spark-gap is turned away from the node, the stronger force now acts under unfavourable conditions against a weaker force, which in this case is acting under favourable conditions. And whether in this latter case the one preponderates or the other, the sparks must necessarily be weaker than in the former case. Thus the change of sign of our phenomenon every quarter wave-length is explained.

Our explanation carries with it a means of further testing its correctness. If it is correct, then the change of sign at the points *B* and *D* should occur in a manner quite different from the change of sign at *C*. At *V, VI,* and *VII* in the figure the circle and the acting forces in these positions are represented, and it is easily seen that if at *B* or *D* we transfer the spark-gap from the one position to the other by rotating the circle within itself, the oscillation changes its direction relatively to a fixed direction within the circle; during this rotation the sparks must therefore become zero either once or an uneven number of times. On the other hand, if the same operation is performed at *C*, the direction of the oscillation does not change; and therefore the sparks must either not disappear at all, or else they must disappear an even number of times. Now when we actually make the experiment, what we observe is this:—At *B* the intensity of the sparks diminishes as soon as we remove the spark-gap from α, becomes zero at the highest point, and again increases to its original value when we come to β. Similarly at *D*. At *C*, on the other hand, the sparks persist without change during the rotation, or, if anything, are somewhat stronger at the highest and lowest points than at those which we have been considering. Furthermore, it strikes the observer that the change of sign ensues after a much smaller displacement at *C*

than at B and D, so that in this respect also there is a contrast between the change at C and that at B and D.

The representation of the electric wave which we have thus sketched can be verified in yet another way, and a very direct one. Instead of placing the plane of our circle in the plane of oscillation, let us place it in the wave-plane; the electric force is now equally strong at all parts of the circle, and for similar positions of the sparks their intensity is simply proportional to this electric force. As might be expected, the sparks are now zero at the highest and lowest points of the circle at all distances, and are strongest at the points along the normal in a horizontal plane. Let us then bring the spark-gap into one of these latter positions, and move slowly away from the wall. This is what we observe:—Just at the conducting metallic surface there are no sparks, but they make their appearance at a very small distance from it; they increase rapidly, are comparatively strong at B, and then again diminish. At C again they are exceedingly feeble, but become stronger as we proceed further. They do not, however, again diminish, but continue to increase in strength, because we are now approaching the primary oscillation. If we were to illustrate the strength of the sparks along the interval $A\,D$ by a curve carrying positive and negative signs, we should obtain almost exactly the curve which has been sketched. And perhaps it would have been better to start from this experiment. But it is not really so striking as the first one described; and furthermore, a periodic change of sign seems to be a clearer proof of wave-motion than a periodic waxing and waning.

We are now quite certain that we have recognised nodes of the electric wave at A and C, and antinodes at B and D. We might, however, in another sense call B and D nodes, for these points are nodes of a stationary wave of magnetic force, which, according to theory, accompanies the electric wave and is displaced a quarter wave-length relatively to it. This statement can be illustrated experimentally as follows:—We again place our circle in the plane of oscillation, but now bring the spark-gap to the highest point. In this position the electric force, if it were homogeneous over the whole extent of the secondary circle, could induce no sparks. It

only produces an effect in so far as its magnitude varies in various parts of the circle, and its integral taken around the circle is not zero. This integral is proportional to the number of lines of magnetic force which flow backwards and forwards through the circle. In this sense, we may say that in this position, the sparks measure the magnetic force, which is perpendicular to the plane of the circle.[1] But now we find that in this position near the wall there is vigorous sparking which rapidly diminishes, disappears at *B*, increases again up to *C*, then again decreases to a marked minimum at *D*, after which it continuously increases as we approach the primary oscillation. Representing the strength of these sparks as ordinates with positive and negative signs, we obtain approximately the dotted line of our figure, which thus represents correctly the magnetic wave. The phenomenon which we first described can also be explained as resulting from the co-operation of the electric and the magnetic force. The former changes sign at the points *A* and *C*, the latter at the points *B* and *D*; thus one part of the action changes sign at each of these points while the other retains its sign; hence the resulting action (as the product) changes sign at each of the points. Clearly this explanation only differs in mode of expression, and not in meaning, from the one first given.

Hitherto we have only considered the phenomena in some of the more important positions of the circle. The number of transitions between these is in a threefold sense infinite. We shall therefore content ourselves with describing the transitions for the case in which the plane of the circle lies in the plane of oscillation. Near the wall the sparking is greatest on the side towards the wall, and least on the opposite side; on rotating the circle within itself the sparking changes from the one value to the other, attaining only intermediate values; there are no zero-points in the circle. As we move away from the wall the sparking on the side remote from it gradually diminishes and becomes zero when the centre of the circle is 1·08 metre distant from the wall; this distance can be ascertained within a few centimetres. As we proceed further, the sparks on the side remote from the wall reappear and at first are still weaker than on the side towards the wall; but

[1] [See Note 18 at end of book.]

the strength of the sparks does not change from the one value to the other simply by passing through intermediate values; on rotating the circle within itself the sparking becomes zero once in the upper and once in the lower half of the circle. The two zero-points develop out of the one which was first formed and separate gradually more from each other, until at B they lie at the highest and lowest points of the circle. By this indication the point B can be determined with fair accuracy, but more exactly still by a further observation of the zero-points. On proceeding further, these zero-points slide over towards the side of the circle facing the wall, approach each other, and again coincide in a single zero-point at a certain distance from the wall which can be sharply determined. In this case the distance of the centre from the wall is 2·35 metres. The point B must lie exactly between this and the analogous point first observed, *i.e.* at a distance of 1·72 metres from the wall; this agrees within a few centimetres with the direct observation. If we proceed further towards C the sparks at all points of the circle tend to become of equal strength, and do become so at C. Beyond C the same performance begins over again. In this region there are no zero-points in the circle. In spite of this the position of the point C can be determined with fair accuracy, inasmuch as in its neighbourhood the phenomena first described alter very rapidly. In my experiments C was 4·10 to 4·15 metres, or say 4·12 metres from the wall. The point D could not be accurately determined for the phenomena had here become very feeble; only this much could be asserted, that its distance from the wall was between 6 and 7·5 metres. For an explanation of the details I may refer to a previous paper.[1] The mathematical developments therein indicated admit of being carried much further; but the experiments seem to be sufficiently intelligible without calculation.

According to our measurement, the distance between B and C is 2·4 metres. If we assume this to be the correct value, the nodal point A lies 0·68 metres behind the wall, the point D 6·52 metres in front of it, which agrees sufficiently well with the experiments. According to this, the half wave-length is 4·8 metres. By an indirect method I had obtained

[1] See V., p. 80.

4·5 metres as the wave-length for the same apparatus. The difference is not so great as to prevent us from regarding the new measurement as confirming the earlier one.[1] If in our earlier measurements we substitute 2·9 for 2·8 metres as the wave-length in the wire, and 7·1 for 7·5 as the length of the coincidence (which will be found to agree with the observations), we can deduce the new value from the earlier observations. Perhaps, indeed, a mean value would be nearest to the truth; and I scarcely think it likely that the nodal point A should lie nearly 0·7 metre behind the metallic wall. Assuming a mean value for the wave-length, and a velocity of propagation equal to that of light, we get for the period of oscillation of our apparatus about 1·55 hundred-millionths of a second, instead of the 1·4 hundred-millionths obtained by calculation.

I have repeated the experiments with some alterations. Altering the distance of the primary oscillation from the reflecting wall did not result in much fresh information. If this distance could have been considerably extended, we might certainly have expected a distinct formation of a second and third wave-length; but there was not sufficient space for such extension. When the distance was diminished the phenomena simply became less interesting, for towards the primary oscillation they were more and more indistinct, and in the opposite direction the reversal of sign became lost. The experiments with an oscillation of different period are better worth describing, for they show that the points which have attracted our attention are determined, not by the form of the wall or of the room, but only by the dimensions of the primary and of the secondary oscillation. I, therefore, used for some experiments a secondary circle of 17·5 cm. radius, and a primary oscillation of the same periodic time as this circle. The primary oscillator was placed at a distance of 8-9 metres from the wall. It is, however, difficult to work with apparatus of such small dimensions. Not only are the sparks exceedingly minute but the phenomena of resonance, etc., are very feebly developed. I suspect that oscillations of such rapidity are very rapidly damped. Thus it was not possible here to make out as much detail as in the case of the larger circle; but the

[1] [See Note 19 at end of book.]

main features, such as those first described above, could be plainly recognised. Near the wall, at distances of 2·5 and 4·5 metres from it, the stronger sparks were on the side next the wall; at the intermediate positions (1·5 and 3·5 metres from the wall) the stronger sparks were on the side next the primary oscillation. A change of sign occurred about every metre; accordingly the half wave-length was here only 2 metres, and the oscillation was more than twice as rapid as that first used.

Finally, I may remark that as far as the above experiments are concerned, no great preparations are essential if one is content with more or less complete indications of the phenomena. After some practice one can find indications of reflection at any wall. Indeed, the action of the reflected waves can be quite well recognised between any one of the iron pillars above referred to and the primary oscillation; and similarly on the opposite side the electromagnetic shadow can be perceived.

Let us now extend our experiments in a new direction. Hitherto the secondary conductor has been placed between the reflecting wall and the primary oscillation—that is to say, in a space in which the direct and reflected waves travel in opposite directions and by interference produce stationary waves. If, on the contrary, we place the primary oscillation between the wall and the secondary conductor, the latter is situated in a space in which the direct and reflected waves travel in the same direction. Hence these must combine to produce a progressive wave, the intensity of which will, however, depend upon the difference of phase between the two interfering waves. If the phenomena are to be at all striking, the two waves must be of similar intensity; hence the distance of the primary oscillation from the wall must not be large compared with the dimensions of the latter, and must be small compared with the distance from the secondary oscillation. In order to test whether the corresponding phenomena could be observed under the working conditions, I arranged an experiment as follows:—The secondary circle was now set up at a distance of 14 metres from the reflecting wall, and therefore 1 metre away from the opposite wall. Its plane was parallel to what we have called the plane of oscillation, and its spark-gap was turned towards the nearer wall so

that the conditions were specially favourable for the appearance of sparks in it. The primary conductor was set up parallel to its original position in front of the conducting wall, and at first at a very short distance—about 30 cm.—from it. The sparks in the secondary circle were extremely feeble. The spark-gap was now adjusted so that no sparks whatever passed over. The primary conductor was next shifted step by step away from the wall. Single sparks soon appeared in the secondary conductor, and these ran into an unbroken stream of sparks when the primary conductor arrived at a distance of 1·5-2 metres from the wall—that is to say, at the point *B*. This might be referred to the decrease in the distance between the two conductors. But when I now removed the primary conductor further away from the wall, and therefore nearer to the secondary, the sparks again diminished and disappeared when the primary arrived at *C*. On proceeding still further the sparks began to increase and did so now continuously. No exact measurement of the wave-length can be deduced from these experiments, but from what has been stated above it will be seen that the wave-lengths already obtained are in accordance with the phenomena. The experiments could be very well carried out with the smaller apparatus. The primary conductor was set up at a distance of 1 metre from the wall, and the corresponding secondary conductor 9 metres from the wall. The sparks in the latter were certainly small, but could be quite well observed. They disappeared when the primary conductor was moved out of its position, whether it was moved towards the wall or towards the secondary conductor. The sparks only reappeared when the distance from the wall was increased to 3 metres, and from there on they did not again disappear on approaching nearer to the secondary conductor. It is worthy of notice that at the same distance of 2 metres the presence of the wall proved to be of assistance in propagating the induction in the case of the slower oscillation, whereas it was a hindrance in the case of the more rapid one. This shows plainly that the position of the points to which we have drawn attention is determined by the dimensions of the oscillator, and not by those of the wall or room.

In acoustics there is an experiment analogous to those last described, in which it is shown that when a tuning-fork is

brought near a wall the sound is strengthened at certain distances and weakened at others. The analogous experiment in optics is Lloyd's form of Fresnel's mirror-experiment.[1] In optics and acoustics these experiments count as arguments in favour of the wave-nature of light and sound; and so the phenomena here described may be regarded as arguments in favour of the propagation of the inductive action of an electric oscillation by wave-motion.

I have described the present set of experiments, as also the first set on the propagation of induction, without paying special regard to any particular theory; and, indeed, the demonstrative power of the experiments is independent of any particular theory. Nevertheless, it is clear that the experiments amount to so many reasons in favour of that theory of electromagnetic phenomena which was first developed by Maxwell from Faraday's views. It also appears to me that the hypothesis as to the nature of light which is connected with that theory now forces itself upon the mind with still stronger reason than heretofore. Certainly it is a fascinating idea that the processes in air which we have been investigating represent to us on a million-fold larger scale the same processes which go on in the neighbourhood of a Fresnel mirror or between the glass plates used for exhibiting Newton's rings.

That Maxwell's theory, in spite of all internal evidence of probability, cannot dispense with such confirmation as it has already received, and may yet receive, is proved—if indeed proof be needed—by the fact that electric action is not propagated along wires of good conductivity with approximately the same velocity as through air. Hitherto it has been inferred from all theories, Maxwell's included, that the velocity along wires should be the same as that of light. I hope in time to be able to investigate and report upon the causes of this conflict between theory and experiment.[2]

[1] [See Note 20 at end of book.] [2] [See Note 21 at end of book.]

IX

THE FORCES OF ELECTRIC OSCILLATIONS, TREATED ACCORDING TO MAXWELL'S THEORY

(*Wiedemann's Ann.* **36**, p. 1, 1889.)

THE results of the experiments on rapid electric oscillations which I have carried out appear to me to confer upon Maxwell's theory a position of superiority to all others. Nevertheless, I based my first interpretation of these experiments upon the older views, seeking partly to explain the phenomena as resulting from the co-operation of electrostatic and electromagnetic forces. To Maxwell's theory in its pure development such a distinction is foreign. Hence I now wish to show that the phenomena can be explained in terms of Maxwell's theory without introducing this distinction. Should this attempt succeed, it will at the same time settle any question as to a separate propagation of electrostatic force, which indeed is meaningless in Maxwell's theory.

Apart from this special aim, a closer insight into the play of the forces which accompany a rectilinear oscillation is not without interest.

The Formulæ

In what follows we are almost solely concerned with the forces in free ether. In this let X, Y, Z be the components of the electric force along the co-ordinates of x, y, z;[1] L, M, N,

[1] Suppose that you are standing at the origin of the system of co-ordinates on the xy-plane. Further assume that the direction of positive x is straight in front, of positive z upwards, and of positive y to the right hand. Unless these conventions were made, the signs of the electric and magnetic forces in the subsequent equations would not have their usual meanings.

the corresponding components of the magnetic force, both being measured in Gauss units;[1] and let t denote the time and A the reciprocal of the velocity of light. Then, according to Maxwell, the time-rate of change of the forces is dependent upon their distribution in space as indicated by the following equations:—

$$(1)\begin{cases} A\frac{dL}{dt} = \frac{dZ}{dy} - \frac{dY}{dz}, \\ A\frac{dM}{dt} = \frac{dX}{dz} - \frac{dZ}{dx}, \\ A\frac{dN}{dt} = \frac{dY}{dx} - \frac{dX}{dy}, \end{cases} \qquad (2)\begin{cases} A\frac{dX}{dt} = \frac{dM}{dz} - \frac{dN}{dy}, \\ A\frac{dY}{dt} = \frac{dN}{dx} - \frac{dL}{dz}, \\ A\frac{dZ}{dt} = \frac{dL}{dy} - \frac{dM}{dx}. \end{cases}$$

Originally, and therefore always, the following conditions must be satisfied:—

$$(3)\ \frac{dL}{dx} + \frac{dM}{dy} + \frac{dN}{dz} = 0, \text{ and } \frac{dX}{dx} + \frac{dY}{dy} + \frac{dZ}{dz} = 0.$$

The electric energy contained in a volume-element τ of the ether is equal to $\frac{1}{8\pi}\int(X^2+Y^2+Z^2)d\tau$; the magnetic energy is equal to $\frac{1}{8\pi}\int(L^2+M^2+N^2)d\tau$, the integration extending through the volume τ. The total energy is the sum of both these portions.

These statements form, as far as the ether is concerned, the essential parts of Maxwell's theory. Maxwell arrived at them by starting with the idea of action-at-a-distance and attributing to the ether the properties of a highly polarisable dielectric medium. We can also arrive at them in other ways. But in no way can a direct proof of these equations be deduced from experience. It appears most logical, therefore, to regard them independently of the way in which they have been arrived at, to consider them as hypothetical assumptions, and to let their probability depend upon the very large number of natural laws which they embrace. If we take up

[1] H. v. Helmholtz, *Wied. Ann.* **17**, p. 48, 1882.

this point of view we can dispense with a number of auxiliary ideas which render the understanding of Maxwell's theory more difficult, partly for no other reason than that they really possess no meaning,[1] if we finally exclude the notion of direct action-at-a-distance.

Multiply equations (1) by L, M, N, and equations (2) by X, Y, Z; add the equations together and integrate over a volume of which $d\tau$ is the volume-element and $d\omega$ the surface-element. We thus get—

$$\frac{d}{dt}\left\{\frac{1}{8\pi}\int(X^2+Y^2+Z^2)d\tau+\frac{1}{8\pi}\int(L^2+M^2+N^2)d\tau\right\}$$
$$=\frac{1}{4\pi A}\int\left\{(NY-MZ)\cos n,x+(LZ-NX)\cos n,y\right.$$
$$\left.+(MX-LY)\cos n,z\right\}d\omega,$$

where n,x n,y n,z denote the angles which the normals from $d\omega$ make with the axis.

This equation shows that the amount by which the energy of the space has increased can be regarded as having entered through the elements of the surface. The amount which enters through each element of the surface is equal to the product of the components of the electric and magnetic forces resolved along the surface, multiplied by the sine of the angle which they form with each other, and divided by $4\pi A$. It is well known that upon this result Dr. Poynting has based a highly remarkable theory on the transfer of energy in the electromagnetic field.[2]

With regard to the solution of the equations we restrict ourselves to the special but important case in which the distribution of the electric force is symmetrical about the z-axis, in such a way that this force at every point lies in the meridian plane passing through the axis of z and only depends upon the z-co-ordinate of the point and its distance $\rho=\sqrt{x^2+y^2}$ from the z-axis. Let R denote the component of the electric force in the direction of ρ, namely $Xx/\rho+Yy/\rho$; and further let P denote the component of the magnetic force

[1] As an example I would mention the idea of a dielectric-constant of the ether.

[2] J. H. Poynting, *Phil. Trans.*, 1884, II. p. 343.

perpendicular to the meridian plane, namely $Ly/\rho - Mx/\rho$. We then assert that if Π is any function whatever of ρ, z, t, which satisfies the equation—

$$A^2 d^2\Pi/dt^2 = \Delta\Pi,$$

and if we put $Q = \rho d\Pi/d\rho$, then the system

$$\rho Z = dQ/d\rho, \qquad \rho P = A\,dQ/dt,$$
$$\rho R = -dQ/dz, \qquad N = 0$$

is a possible solution of our equations.

In order to prove this assertion, we observe that we have—

$$X = R\frac{d\rho}{dx} = -\frac{d^2\Pi}{dx\,dz}, \qquad L = P\frac{d\rho}{dy} = A\frac{d^2\Pi}{dy\,dt},$$

$$Y = R\frac{d\rho}{dy} = -\frac{d^2\Pi}{dy\,dz}, \qquad M = -P\frac{d\rho}{dx} = -A\frac{d^2\Pi}{dx\,dt},$$

$$Z = \frac{1}{\rho}\frac{d}{d\rho}\left(\rho\frac{d\Pi}{d\rho}\right) = \frac{d^2\Pi}{dx^2} + \frac{d^2\Pi}{dy^2}, \qquad N = 0.$$

We have only to substitute these expressions in the equations (1), (2) and (3) to find equations (2) and (3) identically satisfied, and also equations (1) if we have regard to the differential equation for Π.

It may also be mentioned that conversely, neglecting certain limitations of no practical importance, every possible distribution of electric force which is symmetrical about the z-axis can be represented in the above form; but for the purpose of what follows it is not necessary to accept this statement.

The function Q is of importance to us. For the lines in which the surface of revolution $Q = \text{const.}$ cuts its meridian planes are the lines of electric force; if we construct these for every meridian plane at any instant we get a clear representation of the distribution of the force. If we cut the cup-shaped space lying between the surfaces Q and $Q + dQ$ in various places by surfaces of rotation around the z-axis, then for all such cross-sections the product of electric force and cross-section, which Maxwell calls the induction across the section, is the same. If we arrange the system of surfaces $Q = \text{const.}$ so that in passing from one to another Q increases by the same amount dQ, then the same statement holds good if we

compare amongst themselves the cross-sections of the various spaces thus formed. In the plane diagram formed by the intersection of the meridian planes with the equidistant surfaces $Q = \text{const.}$, the electric force is only inversely proportional to the perpendicular distance between two of the lines $Q = \text{const.}$ when the points compared lie at the same distance from the z-axis; in general, the rule is that the force is inversely proportional to the product of this distance, and of the co-ordinate ρ of the point under consideration.

In what follows we shall introduce along with ρ and z the polar co-ordinates r and θ, which are connected with the former by the relations $\rho = r \sin \theta$, $z = r \cos \theta$; r then denotes the distance from the origin of our system of co-ordinates.

The Forces around a Rectilinear Oscillation

Let E denote a quantity of electricity, l a length, $m = \pi/\lambda$ the reciprocal of a length, and $n = \pi/T$ the reciprocal of a time. Let us put

$$\Pi = El \frac{\sin (mr - nt)}{r}.$$

This value satisfies the equation $A^2 d^2\Pi/dt^2 = \Delta\Pi$, if we stipulate that $m/n = T/\lambda = A$, and hence that λ/T shall be equal to the velocity of light. And it must be noticed that the equation referred to is satisfied everywhere, except at the origin of our system of co-ordinates.

In order to find out what electrical processes at this point correspond to the distribution of force specified by Π, let us investigate its immediate neighbourhood. Thus let r be vanishingly small compared with λ, and mr negligible compared with nt. Then Π becomes[1] equal to $-El \sin nt/r$. Now since

$$\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2}\right)\left(\frac{1}{r}\right) = -\frac{d^2}{dz^2}\left(\frac{1}{r}\right),$$

we have

$$X = -d^2\Pi/dxdz, \quad Y = -d^2\Pi/dydz, \quad Z = -d^2\Pi/dzdz.$$

[1] [See Note 22 at end of book.]

Thus the electric forces appear here as the derivatives of a potential—

$$\phi = \frac{d\Pi}{dz} = -\mathrm{E}l \sin nt \frac{d}{dz}\left(\frac{1}{r}\right),$$

and this corresponds to an electrical double-point, whose axis coincides with the z-axis, and whose moment oscillates between the extreme values $+\mathrm{E}l$ and $-\mathrm{E}l$ with the period T. Hence our distribution of force represents the action of a rectilinear oscillation which has the very small length l, and on whose poles at the maximum the quantities of electricity $+\mathrm{E}$ and $-\mathrm{E}$ become free. The magnetic force perpendicular to the direction of the oscillation and in its immediate neighbourhood comes out as—

$$\mathrm{P} = \mathrm{AE}ln \cos nt \sin \theta/r^2.$$

According to the Biot-Savart law this is the force of a current-element of length l lying in the direction of the axis of z, and whose intensity, in magnetic measure, oscillates between the extreme values $+\pi\mathrm{AE/T}$ and $-\pi\mathrm{AE/T}$. In fact, the motion of the quantity of electricity E determines a current of that magnitude.

From Π we get—

$$\mathrm{Q} = \mathrm{E}lm\left\{\cos(mr - nt) - \frac{\sin(mr - nt)}{mr}\right\}\sin^2\theta,$$

and from this the forces Z, R, P follow by differentiation. Now it is true that the formulæ in general turn out to be too complicated to allow of a direct survey of the distribution of the forces. But in some special cases, which we will now indicate, the results are comparatively simple—

(1) We have already considered the immediate neighbourhood of the oscillation.

(2) In the z-axis, *i.e.* in the direction of the oscillation, we have $d\rho = rd\theta$, $dz = dr$, $\theta = 0$; so that here

$$\mathrm{R} = 0, \qquad \mathrm{P} = 0,$$

$$\mathrm{Z} = \frac{2\mathrm{E}lm}{r^2}\left\{\cos(mr - nt) - \frac{\sin(mr - nt)}{mr}\right\}.$$

The electric force acts always in the direction of the oscillation; at small distances it diminishes as the inverse

cube, at greater distances as the inverse square, of the distance.

(3) In the xy-plane, *i.e.* when $z = 0$, we have $dz = -rd\theta$, $d\rho = dr$, $\theta = 90^0$; and therefore—

$$P = \frac{AElmn}{r}\left\{\sin(mr - nt) + \frac{\cos(mr - nt)}{mr}\right\},$$

$$R = 0,$$

$$Z = \frac{Elm^2}{r}\left\{-\sin(mr - nt) - \frac{\cos(mr - nt)}{mr} + \frac{\sin(mr - nt)}{m^2r^2}\right\}.$$

In the equatorial plane through the oscillation the electric force is parallel to the oscillation, and its amplitude is $El\sqrt{1 - m^2r^2 + m^4r^4}/r^3$. The force diminishes continuously with increasing distance, at first rapidly as the inverse cube, but afterwards only very slowly and inversely as the distance itself. At greater distances the action of the oscillation can only be observed in the equatorial plane, and not along the axis.

(4) At very great distances we may neglect higher powers of $1/r$ as compared with lower ones. Thus we have at such distances—

$$Q = Elm\cos(mr - nt)\sin^2\theta,$$

from which we deduce—

$$P = A.Elmn\sin(mr - nt)\sin\theta/r,$$
$$Z = -Elm^2\sin(mr - nt)\sin^2\theta/r,$$
$$R = Elm^2\sin(mr - nt)\sin\theta\cos\theta/r.$$

Whence it follows that $Z\cos\theta + R\sin\theta = 0$. Hence at great distances the force is everywhere perpendicular to the radius vector from the origin of the force; the propagation takes place in the form of a pure transversal wave. The magnitude of the force is $Elm^2\sin(mr - nt)\sin\theta/r$. At a constant distance from the zero-point it decreases towards the axis, being proportional to the distance from the latter.

In order now to find the distribution of force in the remaining parts of space we make use of graphic representation, drawing for definite times the lines of electric force, *i.e.* the curves $Q = \text{const.}$, for equidistant values of Q. Since Q appears as the product of two factors, of which the one

depends only upon r, and the other only upon θ, the construction of these curves presents no great difficulty. We

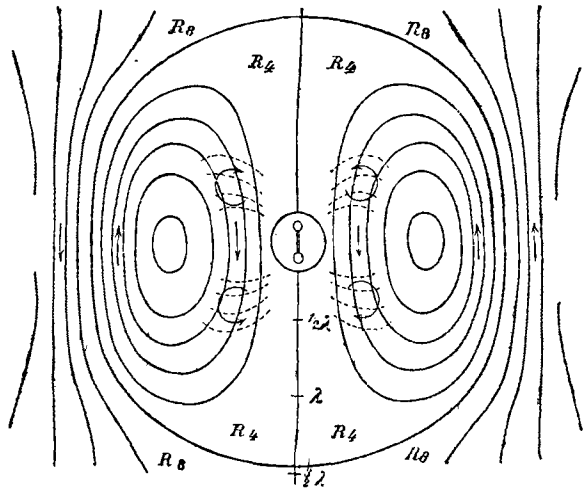


Fig. 27.

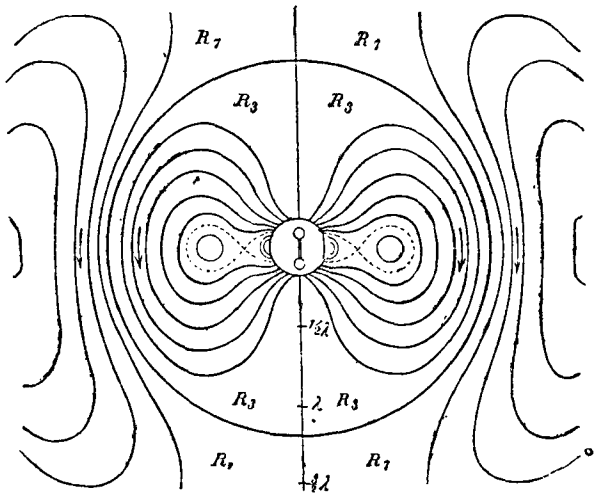


Fig. 30.

split up each value of Q, for which we wish to draw the curve, in various ways into two factors; we determine the angle θ for which $\sin^2\theta$ is equal to the one factor and, by

means of an auxiliary curve, the value of r for which the function of r contained in Q is equal to the other factor; in

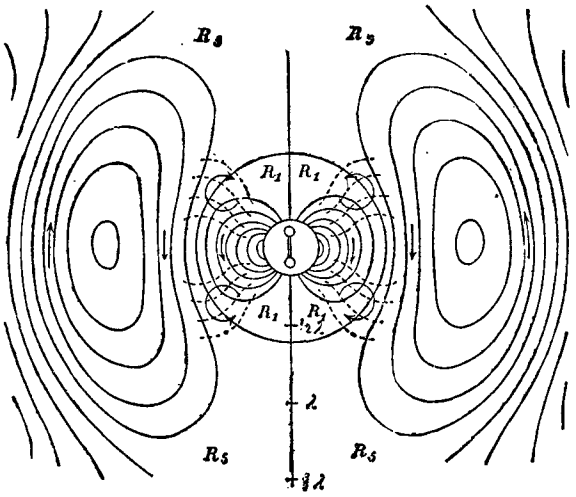


Fig. 28.

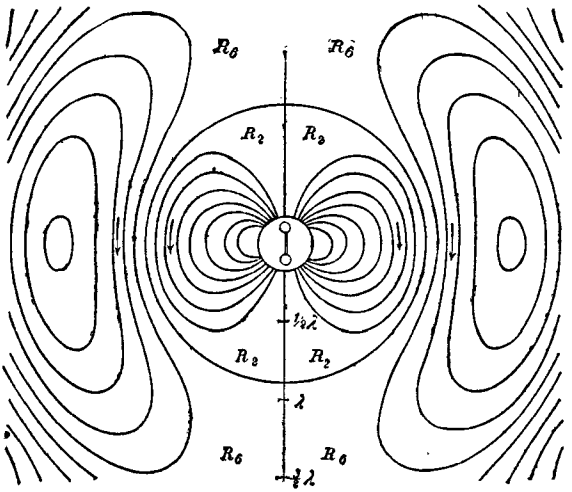


Fig. 29.

this way we find as many points on the curve as we please. On setting about the construction of these curves one perceives many small artifices which it would be tedious to exhibit here.

We shall content ourselves with considering the results of the construction as shown in Figs. 27-30. These figures exhibit the distribution of force at the times $t = 0$, $\frac{1}{4}T$, $\frac{1}{2}T$, $\frac{3}{4}T$, or by a suitable reversal of the arrows for all subsequent times which are whole multiples of $\frac{1}{4}T$. At the origin is shown, in its correct position and approximately to correct scale, the arrangement which was used in our earlier experiments for exciting the oscillations. The lines of force are not continued right up to this picture, for our formulæ assume that the oscillator is infinitely short, and therefore become inadequate in the neighbourhood of the finite oscillator.

Let us begin our explanation of the diagrams with Fig. 27. Here $t = 0$; the current is at its maximum strength, but the poles of the rectilinear oscillator are not charged with electricity—no lines of force converge towards them. But from the time $t = 0$ onwards, such lines of force begin to shoot out from the poles; they are comprised within a sphere represented by the value $Q = 0$. In Fig. 27, indeed, this sphere is still vanishingly small, but it rapidly enlarges, and by the time $t = \frac{1}{4}T$ (Fig. 28) it already fills the space R_1. The distribution of the lines of force within the sphere is nearly of the same kind as that corresponding to a static electric charge upon the poles. The velocity with which the spherical surface $Q = 0$ spreads out from the origin is at first much greater than $1/A$; in fact, for the time $\frac{1}{4}T$ this latter velocity would only correspond to the value of $\frac{1}{4}\lambda$ given in the figure. At an infinitesimal distance from the origin the velocity of propagation is even infinite. This is the phenomenon which, according to the old mode of expression, is represented by the statement that upon the electromagnetic action which travels with the velocity $1/A$, there is superposed an electrostatic force travelling with infinite velocity. In the sense of our theory we more correctly represent the phenomenon by saying that fundamentally the waves which are being developed do not owe their formation solely to processes at the origin, but arise out of the conditions of the whole surrounding space, which latter, according to our theory, is the true seat of the energy. However this may be, the surface $Q = 0$ spreads out further with a velocity which gradually sinks to $1/A$, and by the time $t = \frac{1}{2}T$ (Fig. 29) fills

the space R_2. At this time the electrostatic charge of the poles is at its greatest development; the number of lines of force which converge towards the poles is a maximum. As time progresses further no fresh lines of force proceed from the poles, but the existing ones rather begin to retreat towards the oscillating conductor, to disappear there as lines of electric force, but converting their energy into magnetic energy. Here there arises a peculiar action which can plainly be recognised, at any rate in its beginnings, in Fig. 30 ($t = \frac{3}{4}$T). The lines of force which have withdrawn furthest from the origin become laterally inflected by reason of their tendency to contract together; as this inflection contracts nearer and nearer towards the z-axis, a portion of each of the outer lines of force detaches itself as a self-closed line of force which advances independently into space, while the remainder of the lines of force sink back into the oscillating conductor.

The number of receding lines of force is just as great as the number which proceeded outwards, but their energy is necessarily diminished by the energy of the parts detached. This loss of energy corresponds to the radiation into space. In consequence of this the oscillation would of necessity soon come to rest unless impressed forces restored the lost energy at the origin. In treating the oscillation as undamped, we have tacitly assumed the presence of such forces. In Fig. 27 —to which we now return for the time $t = \mathrm{T}$, conceiving the arrows to be reversed—the detached portions of the lines of force fill the spherical space R_4, while the lines of force proceeding from the poles have completely disappeared. But new lines of force burst out from the poles and crowd together the lines whose development we have followed into the space R_5 (Fig. 28). It is not necessary to explain further how these lines of force make their way to the spaces R_6 (Fig. 29), R_7 (Fig. 30), R_8 (Fig. 27). They run more and more into a pure transverse wave-motion, and as such lose themselves in the distance. The best way of picturing the play of the forces would be by making drawings for still shorter intervals of time and attaching these to a stroboscopic disk.

A closer examination of the diagrams shows that at points which do not lie either on the z-axis or in the xy-plane the direction of the force changes from instant to instant. Thus,

if we represent the force at such a point in the usual manner by a line drawn from the point, the end of this line will not simply move backwards and forwards along a straight line during an oscillation, but will describe an ellipse. In order to find out whether there are any points at which this ellipse approximates to a circle, and in which, therefore, the force turns successively through all points of the compass without any appreciable change of magnitude, we superpose two of the diagrams which correspond to times differing by $\frac{1}{2}T$ from one another, *e.g.* Figs. 27 and 29, or 28 and 30. At such points as we are trying to find, the lines of the one system must clearly cut those of the second system at right angles, and the distances between the lines of the one system must be equal to those of the second. The small quadrilaterals formed by the intersection of both systems must therefore be squares at the points sought. Now, in fact, regions of this kind can be observed; in Figs. 27 and 28 they are indicated by circular arrows, the directions of which at the same time give the direction of rotation of the force. For further explanation dotted lines are introduced which belong to the system of lines in Figs. 29 and 30. Furthermore, we find that the behaviour here sketched is exhibited by the force not only at the points referred to, but also in the whole strip-shaped tract which, spreading out from these points, forms the neighbourhood of the z-axis. Yet the force diminishes in magnitude so rapidly in this direction that its peculiar behaviour only attracts attention at the points mentioned.

In an imperfect series of observations which are not guided by theory, the force-system here described, and required by theory, may well exhibit itself in the manner described in an earlier paper.[1] The observations referred to do not by any means enable us to recognise all the complicated details, but they show correctly the main features of the distribution. According to both observation and theory the distribution of the force in the neighbourhood of the oscillator is similar to the electrostatic distribution. According to both observation and theory the force spreads out chiefly in the equatorial plane and diminishes in that plane at first rapidly, then slowly, without becoming zero

[1] See V., p. 90.

at any intermediate distance. According to both observation and theory the force in the equatorial plane, along the axis, and at great distances, is constant in direction and variable in magnitude; whereas, at intermediate points, its direction varies greatly and its magnitude but little. The only want of accord between theory and the observations referred to is in this—that, according to the former, the force at great distances should always be perpendicular to the radius vector from the origin, whereas, according to the latter, it appeared to be parallel to the oscillation. These two come to the same thing for the neighbourhood of the equatorial plane, where the forces are strongest, but not for directions lying between the equatorial plane and the axis. I believe that the error is on the side of the observations. In the experiments referred to the oscillator was parallel to the two main walls of the room used; and the components of the force parallel to the oscillator might thereby be strengthened as compared with the components normal to the oscillator.

I have therefore repeated the experiments, making various alterations in the position of the primary oscillator, and found that in certain positions the results were in accordance with theory. Nevertheless, the results were not free from ambiguity, for at great distances and in places where the force was feeble, the disturbances due to the environment of the space at my disposal were so considerable that I could not arrive at a trustworthy decision.

While the oscillator is at work the energy oscillates in and out through the spherical surfaces surrounding the origin. But the energy which goes out during each period of oscillation through every surface is greater than that which returns, and is greater by the same amount for all the surfaces. This excess represents the loss of energy due to radiation during each period of oscillation. We can easily calculate it for a spherical surface, whose radius r is so great that we may use the simplified formulæ. Thus the energy which goes out in the element of time dt through a spherical zone lying between θ and $\theta + d\theta$ is

$$dt \,.\, 2\pi r \sin\theta \,.\, rd\theta \,.\, (\mathrm{Z}\sin\theta - \mathrm{R}\cos\theta)\,\mathrm{P} \,.\, 1/4\pi\mathrm{A}.$$

If we here substitute for Z, P, R, the values corresponding to large values of r and integrate with respect to θ from 0 to π, and with respect to t from 0 to T, we get for the energy which goes out through the whole sphere during a half-oscillation

$$\tfrac{1}{3}E^2l^2m^3nt = \pi^4E^2l^2/3\lambda^3.$$

Let us now try to deduce from this an approximate estimate of the quantities actually involved in our experiments. In these we charged two spheres of 15 cm. radius in opposite senses up to a sparking distance of about 1 cm. If we estimate the difference of potential between the two spheres as 120 C.G.S. electrostatic units (gm.$^{\frac{1}{2}}$ cm.$^{\frac{1}{2}}$ sec.$^{-1}$), then each sphere was charged to a potential of $\pm$ 60 C.G.S. units, and therefore its charge was $E = 15 \times 60 = 900$ C.G.S. units (gm.$^{\frac{1}{2}}$ cm.$^{\frac{3}{2}}$ sec.$^{-1}$). Hence the whole stock of energy which the oscillator possessed at the start amounted to $2 \times \frac{1}{2} \times 900 \times 60 = 54{,}000$ ergs, or about the energy which a gramme-weight would acquire in falling through 55 cm. The length l of the oscillator was about 100 cm., and the wave-length about 480 cm. Hence it follows that the loss of energy in the half-period of oscillation was about 2400 ergs.[1] It is therefore evident that after eleven half-oscillations one-half of the energy will have been expended in radiation. The rapid damping of the oscillations, indicated by our experiments, was therefore necessarily determined by the radiation, and would still occur even if the resistance of the conductor and of the spark became negligible.

To furnish energy amounting to 2400 ergs in 1·5 hundred-millionths of a second is equivalent to working at the rate of 22 horse-power. The primary oscillator must be supplied with energy at fully this rate if its oscillations are to be kept up continuously and with constant intensity in spite of the radiation. During the first few oscillations the intensity of the radiation at a distance of about 12 metres from the primary conductor corresponds to the intensity of the sun's radiation at the surface of the earth.

The Interference-Experiments

In order to ascertain the velocity of propagation of the electric force in the equatorial plane, we caused it to interfere

[1] [See Note 23 at end of book.]

with the action of an electric wave proceeding with constant velocity along a wire.[1] It appeared that the resulting interferences did not succeed each other at equal distances, but that the changes were more rapid in the neighbourhood of the oscillation than at greater distances. This behaviour was explained by the supposition that the total force might be split up into two parts, of which the one, the electromagnetic, was propagated with the velocity of light, while the other, the electrostatic, was propagated with a greater, and perhaps infinite velocity. But now, according to our theory, the force under consideration in the equatorial plane is—

$$Z = Elm^3 \left\{ -\frac{\sin(mr-nt)}{mr} - \frac{\cos(mr-nt)}{m^2r^2} + \frac{\sin(mr-nt)}{m^3r^3} \right\},$$

and this expression can in no way be split up into two simple waves travelling with different velocities. Hence if our present theory is correct, the earlier explanation can only serve as an approximation to the truth. Let us now investigate whether the present theory leads to any explanation of the phenomena.

To begin with, we can write $Z = B \sin(nt - \delta_1)$, where the amplitude of the force $B = El\sqrt{1 - m^2r^2 + m^4r^4}/r^3$, and the phase δ_1 of the force is determined by the equation—

$$\tan \delta_1 = \frac{\sin mr/mr + \cos mr/m^2r^2 - \sin mr/m^3r^3}{\cos mr/mr - \sin mr/m^2r^2 - \cos mr/m^3r^3},$$

which, after transformation, gives

$$\delta_1 = mr - \tan^{-1}\frac{mr}{1 - m^2r^2}.$$

In Fig. 31 the quantity δ_1 is represented as a function of mr by the curve δ_1. The length $a\,b$ in the figure corresponds to the value of π, both for abscissæ and ordinates. If we regard r, instead of mr, as the variable abscissa, the length $a\,b$ in the abscissæ corresponds to the half wave-length. For the purpose of referring directly to the experiments which we wish to discuss, there is placed beneath the diagram a further division of the axis of abscissæ into metres. According to the results obtained by direct experiment[2] λ is put = 4·8 metres, and

[1] See VII., p. 107. [2] See VIII., p. 124.

from this the length of the metre (or scale of divisions) is determined; but the first mark of the divided scale is not at the oscillator, but is placed at a distance of 0·45 metre beyond the latter. In this way the divisions represent the divisions

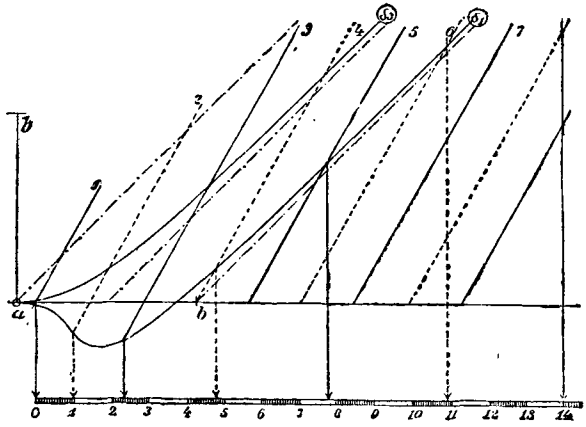


Fig. 31.

of the base-line which we used in determining the interferences. We see from the figure that the phase does not increase from the source; its course is rather as if the waves originated at a distance of about $\frac{1}{2}\lambda$ in space and spread out thence, partly towards the conductor, and partly into space. At great distances the phase is smaller by the value π than it would have been if the waves had proceeded with constant velocity from the origin; the waves, therefore, behave at great distances as if they had travelled through the first half wave-length with infinite velocity.

The action w of the waves in the wire for a given position of the secondary conductor can now be represented in the form $w = C \cdot \sin(nt - \delta_2)$, wherein δ_2 is used as an abbreviation for $m_1 r + \delta = \pi r/\lambda_1 + \delta$. λ_1 denotes the half wave-length of the waves in the wire, which in our experiments was 2·8 metres. δ indicates the phase of its action at the point $r = 0$, which we altered arbitrarily by interposing wires of various lengths. Similarly we were able to alter the amplitude C, and we made it of such magnitude that the action of the waves in the wire was approximately equal to the direct action. The phase of

the interference then depends only upon the difference between the phases δ_1 and δ_2. With that particular adjustment of the secondary circle to which our expression for w relates, both actions reinforce one another (*i.e.* the interference has the sign +) if $\delta_1 - \delta_2$ is equal to zero or an odd multiple of 2π; the actions annual one another (*i.e.* the interference has the sign −) if $\delta_1 - \delta_2$ is equal to π or an odd multiple of it; no interference takes place (the interference has the sign 0) if $\delta_1 - \delta_2$ is equal to an odd multiple of $\frac{1}{2}\pi$.

Let us now suppose that δ is so determined that, at the beginning of the metre-scale, the phase of the interference has a definite value ϵ, so that $\delta_1 = \delta_2 + \epsilon$. The straight line 1 in our figure will then represent the value of $\delta_2 + \epsilon$ as a function of the distance. For the inclination of the line is so chosen that for an increase of abscissa by $\lambda_1 = 2{\cdot}8$ metres, the ordinate increases by the value π; and it is so placed that it cuts the curve δ_1 at a point whose abscissa is at the beginning of the metre-scale. The lines 2, 3, 4, etc., further represent the course of the values of $\delta_2 + \epsilon - \frac{1}{2}\pi$, $\delta_2 + \epsilon - \pi$, $\delta_2 + \epsilon - \frac{3}{2}\pi$, etc. For these lines are parallel to the line 1, and are so drawn that they cut any given ordinate at distances of $\frac{1}{2}\pi$, and any given abscissa at distances of 1·4 metre. If we now project the points of intersection of these straight lines with the curve δ_1 upon the axis of abscissæ below, we clearly obtain those distances for which δ_1 is equal to $\delta_2 + \epsilon + \frac{1}{2}\pi$, $\delta_2 + \epsilon + \pi$, $\delta_2 + \epsilon + \frac{3}{2}\pi$, etc., *i.e.* for which the phase of the interference has increased by $\frac{1}{2}\pi$, π, $\frac{3}{2}\pi$, etc., as compared with the zero-point. We thus deduce directly from the figure the following statements:—If at the zero-point of the base-line the interference has the sign + (−), it first attains the sign 0 at about 1 metre, the sign − (+) at about 2·3 metres, and it again acquires the sign 0 at about 4·8 metres: the interference reverts to the sign + (−) at about 7·6 metres, is again 0 at about 14 metres, and from there on the signs succeed each other in order at about equal distances. If at the zero-point of the base-line the interference has the sign 0, it will also have this sign at about 2·3 metres, 7·6 metres, and 14 metres; it will have a marked positive or negative character at about 1 metre, 4·8 metres, and 11 metres from the zero-point. Intermediate values correspond to intermediate phases. If this

theoretical result is compared with the experimental result, and especially with those interferences which occurred on introducing 100, 250, 400, and 550 cm. of wire,[1] the accordance will be found as complete as could possibly be expected.

I have not been able to account so well for the interferences of the second kind.[2] For producing these interferences we used the secondary circle in a position in which the most important factor was the integral force of induction around the closed circle. If we regard the dimensions of the latter as vanishingly small, the integral force is proportional to the rate of change of magnetic force perpendicular to the plane of the circle, and is therefore proportional to the expression—

$$\frac{dP}{dt} = AEl m^2 n^2 \left\{ -\frac{\cos(mr - nt)}{mr} + \frac{\sin(mr - nt)}{m^2 r^2} \right\}.$$

Hence we deduce the phase δ_3 of this action—

$$\tan \delta_3 = -\frac{\cos mr/mr - \sin mr/m^2 r^2}{\sin mr/mr + \cos mr/m^2 r^2},$$

or, after transformation—

$$\delta_3 = mr - \tan^{-1} mr.$$

The line δ_3 of Fig. 31 represents the course of this function. We see that the phase of this action increases continuously from the origin itself. Hence the phenomena which point to a finite rate of propagation must, in the case of these interferences, make themselves felt even close to the oscillator. This was indeed apparent in the experiments, and therein lay the advantage presented by this kind of interference. But, contrary to the experiment, the apparent velocity near to the oscillator comes out greater than at a distance from it; and it cannot be denied that, according to theory, the change of phase of the interference should be slightly, but noticeably, more rapid than it was in the experiments. It seems to me probable that a more complete theory—in which the two conductors used would not be regarded as vanishingly small,—and perhaps a different estimate of the value of λ, would establish a more satisfactory agreement.

[1] See p. 118. [2] See p. 119.

It is, however, important to notice that even on the basis of Maxwell's theory, the numerical results obtained cannot be explained without assuming a considerable difference between the rates of propagation of the waves in wires and in free space.

Waves in Wire-shaped Conductors

The function $$K(p\rho) = \int_0^\infty e^{-\frac{1}{2}p\rho(e^u + e^{-u})} du,$$

which, for large values of ρ, approximates asymptotically to the function $\sqrt{\pi/2p\rho}\,.\,e^{-p\rho}$, and for infinitesimal values of ρ to the function $-\log(p\rho/2) - 0{\cdot}577$, satisfies the differential equation—

$$\frac{d^2K(p\rho)}{d\rho^2} + \frac{1}{\rho}\frac{dK(p\rho)}{d\rho} - p^2K(p\rho) = 0.$$

If we therefore put—

$$\Pi = \frac{2J}{An}\,.\,\sin\,(mz - nt)\,.\,K(p\rho),$$

then Π satisfies the equation $A^2d^2\Pi/dt^2 = \Delta\Pi$, if we make $p^2 = m^2 - A^2n^2$. Here J must be understood to represent a current expressed in magnetic measure, p and $m = \pi/\lambda$ reciprocals of lengths, and $n = \pi/T$ the reciprocal of a time. The function Π satisfies its equation through all space, except along the z-axis, where it is discontinuous. The values R, Z, P, N, which can be deduced from the above Π, represent therefore an electrical disturbance taking place in a very thin wire stretched along the z-axis. In the immediate neighbourhood of this wire, neglecting quantities which contain even powers of ρ, we have—

$$Q_0 = -\frac{2J}{An}\,.\,\sin\,(mz - nt),$$

and therefore—

$$R_0 = \frac{2Jm}{An\rho}\,.\,\cos\,(mz - nt),$$

$$P_0 = \frac{2J}{\rho}\,.\,\cos\,(mz - nt),$$

in which the suffix 0 indicates that ρ is assumed to be

vanishingly small. From the expression for R_0 it follows that the quantity of free electricity e in unit length of the wire is—

$$e = \frac{1}{4\pi} . 2\pi\rho . R_0 = \frac{Jm}{An} . \cos (mz - nt).$$

Similarly from the expression for P_0 it follows that the current i is—

$$i = \frac{1}{4\pi} . 2\pi\rho . P_0 = J \cos (mz - nt).$$

The values of i and e satisfy of themselves the necessary equation $A de/dt = -di/dz$. They show us that the disturbance under consideration is an electric sine-wave which is propagated in the positive direction along the axis of z, whose half wave-length is λ, and half-period of oscillation T, whose velocity is therefore $\lambda/T = n/m$, and whose intensity is such that the maximum current which arises is $\pm J$.

If we stipulate that external forces may be made to act arbitrarily in the wire, we may regard λ and T as being independent of each other. For every given relation between these quantities, *i.e.* for every given velocity of the waves, the lines of electric force have a definite form which, independently of time, glides along the wire. As before, we represent this form, drawing the lines $Q = \text{const.}$

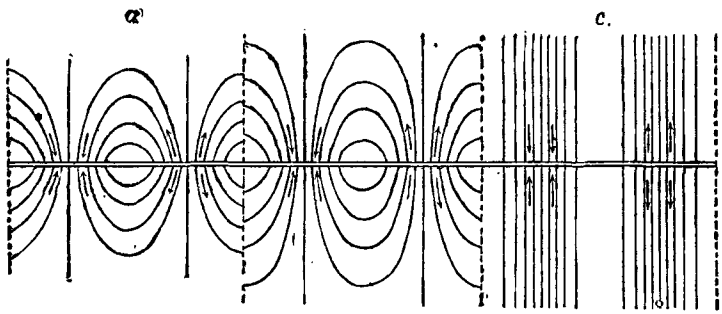


Fig. 32.

Such a representation is carried out in Fig. 32. In the first place, Fig. 32_a represents the case in which the velocity is very small and therefore $p = m$. The drawing then represents a distribution of electrostatic force, viz. that which is obtained when we distribute electricity upon the wire so that its density

is a sine-function of the length of the wire. Fig. 32_b gives the lines of force for a velocity amounting to nearly 28/48 of that of light. We see that in proceeding from and returning to the wire the lines of force make a wider circuit than before. According to the older mode of conception, this would be explained by saying that the electromagnetic force, which is parallel to the wire, weakens the component of the electrostatic force in the same direction, whereas it does not affect the component perpendicular to the wire. The weakening of the component parallel to the wire may even amount to annulling it altogether. For if we take the velocity of propagation of the wire-waves as being equal to that of light, p becomes zero, K $(p\rho)$ reduces to $-\log \rho + \text{const.}$ for every value of ρ, and for every value of ρ—

$$Q = -\frac{2J}{An} \cdot \sin (mz - nt),$$

and therefore—

$$R = \frac{2Jm}{An\rho} \cdot \cos (mz - nt), \; Z = 0,$$

$$P = \frac{2J}{\rho} \cdot \cos (mz - nt), \quad N = 0.$$

The distribution of force then is the simplest that can be conceived; the electric force is everywhere normal to the wire and decreases in inverse proportion to the distance from it. The lines Q = const., drawn for equidistant values of Q, are represented in Fig. 32_c. For waves travelling with a velocity greater than 1/A, p becomes imaginary. For this case our formulæ would require transformation, but as it has no practical significance, we need not discuss it.

At the surface of a conductor, that component of the electric force which is tangential to the surface continues without discontinuity in the interior of the conductor. According to Maxwell, a perfect conductor is understood to mean one in whose interior there can only exist vanishingly small forces. From this it necessarily follows that at the surface of a perfect conductor the components of the force tangential to the surface must vanish. Unless this statement is incorrect, it follows that electric waves in wires of good conductivity must be propagated with the velocity of light and in the form which is

represented in Fig. 32_c. For only in this particular force-distribution is the force everywhere normal to the surface of the wire. In fact, then, it follows from Maxwell's theory, as well as from the older theories, that electric waves travel along perfectly conducting wires with the velocity of light.

If, on the other hand, we are to place any reliance upon our experiments, this conclusion is incorrect—the propagation takes place with a much smaller velocity and in some such form as is indicated in Fig. 32_b. The result is all the more remarkable, because the velocity in wires appears likewise to be a velocity which is quite independent of the nature of the wire. I have found it to be the same in wires of the most diverse metals, varying widely in thickness and in the shape of their cross-section, and also in columns of conducting fluids. The causes which determine this velocity still remain obscure. The resistance, at all events, has nothing to do with it. For some time I thought that it might be affected by the constant k, through the introduction of which Hr. H. v. Helmholtz has extended Maxwell's theory;[1] but further consideration led to the rejection of this idea. If only the limiting condition were correct, a wave of the form of Fig. 32_c would yet be possible. This would always be a pure transversal wave, and as such must travel with the same velocity as plane transversal waves in space, whether simultaneous longitudinal waves are possible or not. Although a finite value of the constant k would not explain the difference between the two velocities, it would postulate the possibility of two kinds of waves in the wire with different velocities: experiment has hitherto given no intimation of such a phenomenon. It seems rather to be doubtful whether the limiting condition is correct for rapidly alternating forces.

Although it does not appear to be possible, on the one hand, to confer a velocity of any desired magnitude upon the waves travelling along the z-axis, there is no difficulty, on the other hand, in reducing the velocity as much as may be desired below its maximum value, or in producing distributions of force intermediate between the forms 32_a and 32_b. With this object the waves are made to proceed along crooked wires or wires rolled into spirals. For example, I rolled a wire 40 metres

[1] H. v. Helmholtz, *Ges. Abh.* 1, p. 545.

long into a spiral 1 cm. in diameter, and so tightly that the length of the spiral was 1·6 metre; in this I was able to observe nodes at distances of about 0·31 metre, whereas, in the straight wire, the nodes were 2·8 metres apart. As the spiral was stretched out, the one value changed gradually into the other. Hence, when the velocity is measured along the z-axis (the axis of the spiral), the wave moves much more slowly in the coiled wire. When the velocity is measured along the wire itself, on the other hand, the wave certainly moves more rapidly. Along crooked wires the behaviour is similar. Unless I am mistaken, Maxwell's theory, assuming the limiting condition for good conductors, is unable to account for this. It seems to me that according to this theory the propagation, measured along the z-axis, must for every form of conductor take place with the velocity of light; provided, in the first place, that the resistance of the conductor does not come into consideration, and, in the second place, that the dimensions of the conductor perpendicular to the axis are negligible in comparison with the wave-length. Both conditions are satisfied by coiled metallic wires; but what should happen does not happen.

In our endeavour to explain the observations by means of Maxwell's theory, we have not succeeded in removing all difficulties. Nevertheless, the theory has been found to account most satisfactorily for the majority of the phenomena; and it will be acknowledged that this is no mean performance. But if we try to adapt any of the older theories to the phenomena, we meet with inconsistencies from the very start, unless we reconcile these theories with Maxwell's by introducing the ether as dielectric in the manner indicated by v. Helmholtz.

X

ON THE PROPAGATION OF ELECTRIC WAVES BY MEANS OF WIRES

(*Wiedemann's Ann.* **37**, p. 395, 1889.)

WHEN a constant electric current flows along a cylindrical wire, its strength is the same at every part of the section of the wire. But if the current is variable, self-induction produces a deviation from this most simple distribution. For the central parts of the wire are, on the whole, less distant from all the rest than are the outer parts; hence induction opposes variations of the current in the centre of the wire more strongly than at the circumference, and consequently the current by preference flows along the outer portion of the wire. When the current changes its direction a few hundred times per second, the deviation from the normal distribution can no longer be imperceptible. This deviation increases rapidly with the rate of alternation; and when the current alternates many million times per second, almost the whole of the interior of the wire must, according to theory, appear free from current, and the flow must confine itself to the very skin of the wire. Now in such extreme cases the above conception of what takes place is obviously not free from physical difficulties; and preference must be given to another conception of the matter which was first presented by Messrs. O. Heaviside[1] and J. H. Poynting,[2] as the correct interpretation of Maxwell's equations as applied to this case. According to this view, the electric force which determines the current is not propagated

[1] O. Heaviside, *Electrician*, January 1885, *Phil. Mag.* **25**, p. 153, 1888.
[2] J. H. Poynting, *Phil. Trans.* **2**, p. 277, 1885.

in the wire itself, but under all circumstances penetrates from without into the wire, and spreads into the metal with comparative slowness and according to laws similar to those which govern changes of temperature in a conducting body. Thus when the forces around the wire continually alter their direction, the action of these forces only extends to a very slight depth within the metal; the slower the fluctuations, the more deeply will the action penetrate; and lastly, when the changes succeed each other with infinite slowness, the force has time to penetrate into the interior of the wire and to act with uniform strength throughout it.[1]

Whatever conception we may form from the theoretical results, it is important to find out whether these latter agree with the actual facts. Inasmuch as I made use of electric waves in wires of exceedingly short period in my experiments on the propagation of electric force, it was natural to test by means of these the correctness of the conclusions deduced. As a matter of fact the theory was found to be confirmed by the experiments which are now to be described; and it will be seen that these few experiments are amply sufficient to support the conception introduced by Messrs. Heaviside and Poynting. Similar experiments, with similar results, have been carried out by Dr. O. J. Lodge,[2] who has, however, used quite different experimental methods, and mainly with the object of elucidating the theory of lightning-conductors. To what extent the conclusions are true which were deduced by Dr. Lodge in the latter respect from his experiments must, in the first place, depend upon the actual rapidity of succession of the changes of electrical conditions which accompany lightning.

The apparatus and methods which are here mentioned are those which have been fully described in my previous papers. The waves used were such as had in wires nodes about 3 metres apart.

1. When a primary conductor acts through air upon a secondary conductor, there can be no doubt that the action penetrates from without into the latter. For it may be regarded as an established fact that in air the action is propagated from point to point, and it must therefore first meet

[1] [See Note 24 at end of book.]

[2] O. J. Lodge, *Journ. of Soc. of Arts*, May 1888; *Phil. Mag.* **26**, p. 217, 1888.

the outer boundary of the conductor before it can act upon the inside. Now it can be shown that a closed metallic envelope does not allow the action to pass through it at all. If we place the secondary conductor in a favourable position with reference to the primary so that sparks 5-6 mm. long are obtained, and then surround it with a closed box of sheet zinc, not the slightest amount of sparking can be detected. Similarly the sparks disappear when the primary conductor is completely surrounded by a metal box. It is known that a metal screen does not interfere with the integral force of induction when the fluctuations of current are relatively slow. At first sight this appears to contradict the above experimental results. But the contradiction is only apparent and disappears when the time-relations are considered. In a similar way a badly-conducting envelope protects its interior completely against rapid fluctuations of external temperature, less completely against slow fluctuations, and not at all against a permanent rise or fall in temperature. The thinner the envelope the more rapid are the fluctuations which can act through it upon the interior. And so in our case also, the electric action should clearly penetrate into the interior if we only reduced sufficiently the thickness of the metal. Yet I did not find it easy to secure the requisite thinness. A box covered with tinfoil acted as a perfect screen; and so too did a box of gilt paper when care was taken to make good contact between the edges of the separate pieces of paper. In this case the thickness of the conducting metal could scarcely be estimated as high as $\frac{1}{20}$ mm. I next fitted the protecting envelope as closely as possible around the secondary conductor. For this purpose its spark-gap was drawn out to about 20 mm.; and, in order to be still able to detect electric disturbances in it, an auxiliary spark-gap was introduced just opposite the usual one. The sparks in this were not so long as in the proper spark-gap because the resonance-effect was now absent, but they still were quite vigorous. After being thus prepared the conductor was completely surrounded with a tube-shaped conducting envelope made as thin as possible; this did not touch the conductor, but was brought as close as possible to it, and in the neighbourhood of the auxiliary spark-gap—in order to be able to make use of the latter—was made of wire-gauze.

Between the poles of this envelope the sparking was as vigorous as it had previously been in the secondary conductor itself; but in the enclosed conductor not the slightest electrical disturbance could be perceived. It does not interfere with the result if the envelope touches the conductor at a few points; it is not necessary to insulate the two from one another in order to make the experiment succeed, but only in order to give it its demonstrative force. In imagination we can clearly draw the envelope around the conductor more closely than is possible in practice; indeed, we can imagine it to coincide with the outer skin of the conductor. Thus although the electrical disturbances at the surface of our conductor are so powerful as to produce sparks of 5-6 mm. length, yet at a depth of only $\frac{1}{20}$ mm. below its surface there is such complete calm that not the slightest sparking is produced. We are thus led to suspect that what we call an induced current in the secondary conductor is a process which takes place for the most part in the surrounding space and in which the inside of the conductor scarcely plays any part.

2. We might admit that this is so when an electrical disturbance passes through a dielectric, but yet maintain that it is otherwise when the disturbance, as we usually say, has been propagated in a conductor. Near one of the end plates of our primary conductor let us place a conducting plate and fasten to it a long straight wire; in our earlier experiments we have already shown how the action of the primary oscillation can be conveyed to great distances with the aid of such a wire. The usual view of this is that the wave proceeds through the wire. We will, however, try to show that all the changes are confined to the surface and the space outside, and that the interior of the wire knows nothing of the passing waves. I first arranged experiments in the following manner. From the conducting wire a piece 4 metres long was removed and replaced by two strips of zinc sheet 4 metres long and 10 cm. broad, which were laid flat one upon the other with their ends touching and firmly connected. Along the whole length of the middle line between the strips, and hence almost completely surrounded by metal, was placed a copper wire 4 metres long and covered with gutta-percha. It made no difference in the experiments whether the outer ends of this wire were in metallic connection with

the strips, or insulated from them; but generally the ends were soldered to the zinc strips. The copper wire was cut in the middle, and its ends were twisted round each other and led out between the strips to a small spark-gap by which any electrical disturbance in the wire could be perceived. Not the slightest action could be detected at the spark-gap, even when the strongest possible waves were led through the whole arrangement. But if any part of the copper wire, a few decimetres long, was pulled out of its place so as to project but a little beyond the strips, sparking immediately began. The longer the projecting part and the further it extended beyond the edge of the strips, the more vigorous became the sparking. The absence of sparks in the first instance cannot be attributed to any unfavourable conditions of resistance; no change has taken place in these conditions; only the wire at first was inside a conducting mass and beyond the reach of outside influences. Indeed, it is only necessary to enclose the projecting part of the wire with a little tinfoil in metallic connection with the zinc strips, in order to stop the sparking at once. By so doing we virtually bring the copper wire back inside the conductor. In like manner the sparks become weaker if we carry another wire, in a somewhat larger arc, around the part of the gutta-percha wire which projects beyond the strips; the second wire cuts off from the first part of the external effect. Indeed, we may say that the edge of the zinc strip itself in a similar way cuts off from the middle of the strip some of the induction. For if we now remove one of the two zinc strips and simply let the gutta-percha wire rest upon the other, we always perceive sparks in the wire; but these are extremely feeble in the middle of the strip, and much more powerful towards the edge. Just as electricity when distributed by electrostatic induction would tend to accumulate on the sharp edge of the strip, so here the current seems by preference to move along the edge. In both cases we may say that the outer parts protect the inner from an influence exerted from the outside.

Equally convincing, and somewhat neater, are the following experiments:—I inserted in the conductor, along which the waves were transmitted, a very thick copper wire 1·5 metre long, whose ends carried two circular metallic disks 15 cm.

in diameter. The wire passed through the centres of the disks whose planes were perpendicular to it. Around the edge of each disk were twenty-four equidistant holes. A spark-gap was inserted in the wire. When the waves traversed the wire they produced sparks up to 6 mm. in length. A thin copper wire was now stretched across between two corresponding holes in the disks. The sparking distance thereupon sank to 3·2 mm. No other change was produced when the thin wire was replaced by a thick one, or when twenty-four wires were used instead of the single one, provided they were drawn all together through the same pair of holes. But it was otherwise when the wires were distributed around the edge of the disks. When a second wire was added opposite the first one the spark-length sank to 1·2 mm. When two more wires were put in midway between the others, the spark-length went as low as 0·5 mm. The insertion of four more wires in intermediate positions left sparks barely 0·1 mm. long; and after all twenty-four wires were inserted at equal distances no sparking whatever could be perceived inside. Yet the resistance of the inner wire was much smaller than the joint resistance of all the outer wires; and furthermore, we have given a special proof that the resistance is of no consequence. At the side of the tube of wires which has been built up let us place as a shunt a conductor precisely similar to the one inside the tube; we see vigorous sparking in the former, but none whatever in the latter. The former is not screened, the latter is screened by the tube of wires. We have here an electromagnetic analogue to the electrostatic experiment known under the name of the electric bird-cage.

Again I altered the experiment in the way indicated in Fig. 33. The two disks were moved nearer together so that,

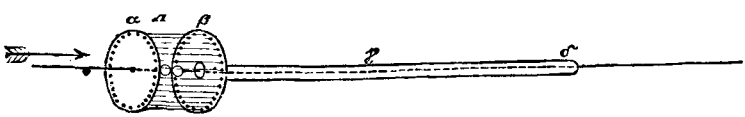


Fig. 33.

with the wires stretched between them, they formed a wire-cage A just big enough to contain the spark-micrometer. One of the disks (α) remained in metallic connection with the central wire; the other (β) was insulated from it by cutting

out a round hole, and was instead connected with a conducting tube γ which, without touching the central wire, completely surrounded it for a distance of 1·5 metre. The free end δ of the tube was then placed in metallic connection with the central wire. The wire with its spark-gap still lies in a space surrounded by a metallic screen; and it seems to follow naturally after what has already been stated that, whether the waves be led through the arrangement in the one direction or the other, not the slightest electrical disturbance would be detected in the wire. So far, then, this arrangement offers nothing new; but it has this advantage over the preceding one, that we can replace the protecting metal tube γ by others with thinner and thinner walls, and so find out what thickness of wall is just sufficient to stop off the outside action. Very thin brass tubes—tubes of tinfoil and tubes of Dutch metal—acted as perfect screens. I next took glass tubes which had been chemically silvered, and now found it quite easy to prepare tubes so thin that, in spite of their protection, there was vigorous sparking in the central wire. But the sparks only appeared when the film of silver was so thin that it was no longer quite opaque to light, and was certainly thinner than $\frac{1}{1000}$ mm. In imagination, though not in practice, we may draw the protecting envelope more and more closely around the wire, until at last it coincides with its surface; and we may feel certain that nothing would practically be altered thereby. So, however vigorously the waves may really play about the wire, inside it is perfectly calm; the action of the waves scarcely penetrates further into the wire than does the light which is reflected from its surface. Hence we should expect to find the real seat of these waves in the neighbourhood of the wire, and not in the wire itself; and instead of saying that our waves are propagated in the wire, we should rather say that they glide towards and along the wire.

Instead of inserting the arrangement last described in the conducting wire in which waves were indirectly produced, we can insert it in one of the branches of the primary conductor itself. In such experiments I obtained much the same results as in the previous ones. Hence it must be equally true of our primary oscillation, that its seat is not to be found in the

interior of the conductor; only the outer skin of the conductor, about which it plays, takes part in it.[1]

One further item of information may be added to what we have already learned about waves in wires, and this relates to the method of carrying out the experiments. If our waves have their seat in the space surrounding the wire, then a wave gliding along a single wire will not be propagated through the air alone; but, inasmuch as its action extends to a considerable distance, it will be propagated in the neighbouring walls, the floor, etc., and so will develop into a complicated phenomenon. But if we set up in exactly the same way two auxiliary plates opposite the two poles of our primary conductor, connect a wire to each of them, and lead both wires straight and parallel to one another to the same distance, then the action of the waves makes itself felt only in the neighbourhood of the space between the two wires. Hence it is only in the space between the wires that the wave progresses. We can thus take measures to secure that the propagation occurs only through air or another insulator, and by so arranging matters can experiment more conveniently and with less fear of complications. The wave-lengths thus obtained are, however, approximately the same as those obtained with single wires; so that even with the latter the disturbing effects do not seem to be of much importance.

3. From what has already been stated, we may conclude that rapid electric oscillations are quite incapable of penetrating metallic layers of any thickness, and that it is therefore quite impossible to excite sparks by the aid of such oscillations inside closed metallic envelopes. Hence, if we see sparks induced by such oscillations inside metallic envelopes which are nearly, but not quite, closed, we must conclude that the electric disturbance has penetrated through the existing openings. And this mode of conception is the correct one; but in some cases it contradicts the usual view so completely that special experiments are required to induce us to forsake the usual view for the newer one. We shall select a striking case of this kind; and by making certain of the correctness of our

[1] The calculation of the self-induction of such conductors on the assumption of uniform current-density in the interior must therefore lead to totally unreliable results. It is surprising that the results obtained under such erroneous assumptions should yet appear to agree approximately with the truth.

view in this case, we shall show its probability in all other cases. We return to the arrangement described in the previous section, and represented in Fig. 33, only we no longer connect the protecting tube at δ with the central wire. We now send the train of waves through the arrangement in the direction from A towards δ. We obtain brilliant sparks at A, and these are about as strong as if we had inserted the spark-gap in the conducting wire without any protection. Nor do the sparks become much smaller if, without altering anything else, we lengthen the tube γ considerably—up to about 4 metres. According to the usual view, it would be said that the wave on reaching A easily passes through the thin metal disk a, which is a good conductor, then springs across the spark-gap at A and proceeds along the central wire. According to our conception, on the other hand, we ought to describe what happens as follows:—The wave on reaching A is absolutely unable to pass through the metal disk, so it glides along the disk over the outside of the apparatus, travelling along thus until it reaches the point δ, 4 metres off. Here it divides—one part, which at present does not concern us, immediately proceeds straight along the wire; another part bends round into the inside of the tube and runs back the whole 4 metres in the air-space between the tube and the central wire, until it reaches the spark-gap A, where it now produces sparks. We shall show by the following experiments that our conception, although somewhat complicated, is yet the correct one. In the first place, every trace of sparking at A disappears as soon as we close the opening at δ, even if it be only with a tinfoil cap. Our waves have a wave-length of only 3 metres; before their action has reached the point δ, it has gone back to zero at A, and has changed sign. What influence then could the closing of the distant opening at δ exert upon the spark at A, if the latter really appears as soon as the wave passes through the metallic partition? In the second place, the sparks disappear when we make the central wire end inside the tube γ, or at the open end δ of this tube; they reappear when the end of the wire is allowed to project beyond the opening, even if only for 20-30 cm. What influence could this insignificant lengthening of the wire have upon the spark at A, unless the projecting end of the wire were just the means by which a part

of the wave is intercepted and brought through the opening δ into the interior? Let us, in the third place, introduce a second spark-gap B in the central wire between A and δ, and surround it with a wire-cage just like A. When we place the poles in B so far apart that sparks can no longer pass, we find that it is no longer possible to obtain any perceptible sparks in A. But if, in like manner, we hinder the passage of the sparks in A, we find that this has scarcely any influence upon the sparks in B. Hence, for the passage of the sparks in A, it is requisite that they should first pass in B; but for the passage of sparks in B, it is not necessary that they should first pass in A. Hence the direction of propagation in the interior is from B towards A, not from A towards B.

Moreover, we can adduce other and more convincing proofs. By making the spark-gap either vanishingly small or very great, we may prevent the wave returning from δ towards A from expending its energy in the formation of sparks. In this case the wave will be reflected at A, and will again proceed from A towards δ. But in doing so it must combine with the direct waves to form stationary oscillations with nodes and antinodes. If we succeed in showing the presence of these, we can no longer have any doubt as to the correctness of our conception. For this purpose we must necessarily give our apparatus somewhat different dimensions, so as to be able to introduce electric resonators inside it. I therefore carried the central wire through the axis of a cylindrical tube 5 metres long and 30 cm. in diameter. This tube was not

Fig. 34.

made of solid metal, but was built up of twenty-four copper wires; these were stretched parallel to one another along the generating surface over seven equidistant circular rings of stout wire, as indicated in Fig. 34. The resonator to be used I made as follows:—Copper wire 1 mm. thick was coiled tightly into a spiral of 1 cm. diameter. About 125 turns of this were taken, pulled out a little, and bent into a circle of 12 cm. diameter; between the free ends was inserted an

adjustable spark-gap. Special experiments had shown that this circle was in resonance with the waves of 3 metres length in the wire, and yet it was sufficiently small to be introduced between the central wire and the tube. Both ends of the tube were at first left open, and the resonator was held inside in such a way that its plane included the central wire, and the spark-gap was not turned exactly inwards or outwards, but faced towards one or other end of the tube; vigorous sparks, $\frac{1}{2}$-1 mm. long, appeared at the spark-gap. If now both ends of the tube were closed by four wires arranged crosswise and connected with the central conductor, not the slightest sparking could be discovered inside, which proves that the network of the tube is sufficiently close for our experiments. In the next place, the cross-wires on the β side of the tube (*i.e.* the side remote from the origin of the waves) were removed. No sparks could be detected when the resonator was in the immediate neighbourhood of the remaining cross-wires, *i.e.* in the position α, which corresponds to the spark-gap A of our earlier experiments. But when it was moved from this position towards β the sparks reappeared, became very vigorous at a distance of 1·5 metre from α, decreased again and almost completely disappeared at a distance of 3 metres, and again became stronger towards the end of the tube. Our supposition is therefore confirmed. It is right that there should be a node at the closed end, for at the metallic connection between the central wire and the tube the electric force between both must necessarily be zero. It is otherwise if we cut the central conductor at this point, quite near the cross-wires, leaving a gap a few centimetres long. In this case the wave is reflected with the opposite phase, and we should expect an antinode at α. And, in fact, we do now find vigorous sparks in the resonator; these, however, rapidly become smaller as we move from α towards β, disappear almost entirely at a distance of 1·5 metre, become vigorous again at a distance of 3 metres, and finally give plain indications of a second node at a distance of 4·5 metres, *i.e.* 0·5 metre from the open end of the tube. The nodes and antinodes which we have described lie at fixed distances from the closed end, and alter their position as this does; but otherwise they are quite independent of processes going on outside the tube, *e.g.* of the nodes and antinodes which

we may produce there. The phenomena occur in precisely the same way when we allow the waves to traverse the tube in the opposite direction, *i.e.* from the open to the closed end; but this case is less interesting, because the mode of propagation of the waves here differs less from the usual conception than in the case which we have just discussed. If we leave the central wire uncut, and both ends of the tube open, and produce in the whole system stationary waves, with nodes and antinodes, we always find a node inside the tube corresponding to each node outside, which proves that the rate of propagation is approximately the same inside and outside.

On studying the experiments above described, the mode in which we have interpreted them, and the explanations of the investigators referred to in the introduction, one difference will be found especially striking between the conception here advocated and the usually accepted view. In the latter conductors appear as the only bodies which take part in the propagation of electrical disturbances—non-conductors as bodies which oppose this propagation. According to our conception, on the other hand, all propagation of electrical disturbances takes place through non-conductors; and conductors oppose this propagation with a resistance which, in the case of rapid alternations, is insuperable. We might almost feel inclined to agree to the statement that conductors and non-conductors should, according to this conception, have their names interchanged. Such a paradox, however, only arises because we omit to specify what conduction or non-conduction is under discussion. Undoubtedly metals are non-conductors for electric force, and for this very reason they, under certain conditions, restrain it from becoming dissipated, and compel it to remain concentrated; they thus become conductors of the apparent source of these forces—the electricity—to which the usual terminology has reference.

XI

ON ELECTRIC RADIATION

(*Sitzungsber. d. Berl. Akad. d. Wiss.* Dec. 13, 1888. *Wiedemann's Ann.* **36**, p. 769.)

As soon as I had succeeded in proving that the action of an electric oscillation spreads out as a wave into space, I planned experiments with the object of concentrating this action and making it perceptible at greater distances by putting the primary conductor in the focal line of a large concave parabolic mirror. These experiments did not lead to the desired result, and I felt certain that the want of success was a necessary consequence of the disproportion between the length (4-5 metres) of the waves used and the dimensions which I was able, under the most favourable circumstances, to give to the mirror. Recently I have observed that the experiments which I have described can be carried out quite well with oscillations of more than ten times the frequency, and with waves less than one-tenth the length of those which were first discovered. I have, therefore, returned to the use of concave mirrors, and have obtained better results than I had ventured to hope for. I have succeeded in producing distinct rays of electric force, and in carrying out with them the elementary experiments which are commonly performed with light and radiant heat. The following is an account of these experiments:—

The Apparatus

The short waves were excited by the same method which we used for producing the longer waves. The primary

conductor used may be most simply described as follows:— Imagine a cylindrical brass body,[1] 3 cm. in diameter and 26 cm. long, interrupted midway along its length by a spark-gap whose poles on either side are formed by spheres of 2 cm. radius. The length of the conductor is approximately equal to the half wave-length of the corresponding oscillation in straight wires; from this we are at once able to estimate approximately the period of oscillation. It is essential that the pole-surfaces of the spark-gap should be frequently repolished, and also that during the experiments they should be carefully protected from illumination by simultaneous side-discharges; otherwise the oscillations are not excited. Whether the spark-gap is in a satisfactory state can always be recognised by the appearance and sound of the sparks. The discharge is led to the two halves of the conductor by means of two gutta-percha-covered wires which are connected near the spark-gap on either side. I no longer made use of the large Ruhmkorff, but found it better to use a small induction-coil by Keiser and Schmidt; the longest sparks, between points, given by this were 4·5 cm. long. It was supplied with current from three accumulators, and gave sparks 1-2 cm. long between the spherical knobs of the primary conductor. For the purpose of the experiments the spark-gap was reduced to 3 mm.

Here, again, the small sparks induced in a secondary conductor were the means used for detecting the electric forces in space. As before, I used partly a circle which could be rotated within itself and which had about the same period of oscillation as the primary conductor. It was made of copper wire 1 mm. thick, and had in the present instance a diameter of only 7·5 cm. One end of the wire carried a polished brass sphere a few millimetres in diameter; the other end was pointed and could be brought up, by means of a fine screw insulated from the wire, to within an exceedingly short distance from the brass sphere. As will be readily understood, we have here to deal only with minute sparks of a few hundredths of a millimetre in length; and after a little practice one judges more according to the brilliancy than the length of the sparks.

The circular conductor gives only a differential effect, and

[1] See Figs. 35 and 36 and the description of them at the end of this paper.

is not adapted for use in the focal line of a concave mirror. Most of the work was therefore done with another conductor arranged as follows:—Two straight pieces of wire, each 50 cm. long and 5 mm. in diameter, were adjusted in a straight line so that their near ends were 5 cm. apart. From these ends two wires, 15 cm. long and 1 mm. in diameter, were carried parallel to one another and perpendicular to the wires first mentioned to a spark-gap arranged just as in the circular conductor. In this conductor the resonance-action was given up, and indeed it only comes slightly into play in this case. It would have been simpler to put the spark-gap directly in the middle of the straight wire; but the observer could not then have handled and observed the spark-gap in the focus of the mirror without obstructing the aperture. For this reason the arrangement above described was chosen in preference to the other which would in itself have been more advantageous.

The Production of the Ray

If the primary oscillator is now set up in a fairly large free space, one can, with the aid of the circular conductor, detect in its neighbourhood on a smaller scale all those phenomena which I have already observed and described as occurring in the neighbourhood of a larger oscillation.[1] The greatest distance at which sparks could be perceived in the secondary conductor was 1·5 metre, or, when the primary spark-gap was in very good order, as much as 2 metres. When a plane reflecting plate is set up at a suitable distance on one side of the primary oscillator, and parallel to it, the action on the opposite side is strengthened. To be more precise:—If the distance chosen is either very small, or somewhat greater than 30 cm., the plate weakens the effect; it strengthens the effect greatly at distances of 8-15 cm., slightly at a distance of 45 cm., and exerts no influence at greater distances. We have drawn attention to this phenomenon in an earlier paper, and we conclude from it that the wave in air corresponding to the primary oscillation has a half wave-length of about 30 cm. We may expect to find a still further reinforcement if we replace the plane surface by a concave mirror having the form of a

[1] See V., VII., VIII.

parabolic cylinder, in the focal line of which the axis of the primary oscillation lies. The focal length of the mirror should be chosen as small as possible, if it is properly to concentrate the action. But if the direct wave is not to annul immediately the action of the reflected wave, the focal length must not be much smaller than a quarter wave-length. I therefore fixed on $12\frac{1}{2}$ cm. as the focal length, and constructed the mirror by bending a zinc sheet 2 metres long, 2 metres broad, and $\frac{1}{2}$ mm. thick into the desired shape over a wooden frame of the exact curvature. The height of the mirror was thus 2 metres, the breadth of its aperture 1·2 metre, and its depth 0·7 metre. The primary oscillator was fixed in the middle of the focal line. The wires which conducted the discharge were led through the mirror; the induction-coil and the cells were accordingly placed behind the mirror so as to be out of the way. If we now investigate the neighbourhood of the oscillator with our conductors, we find that there is no action behind the mirror or at either side of it; but in the direction of the optical axis of the mirror the sparks can be perceived up to a distance of 5-6 metres. When a plane conducting surface was set up so as to oppose the advancing waves at right angles, the sparks could be detected in its neighbourhood at even greater distances—up to about 9-10 metres. The waves reflected from the conducting surface reinforce the advancing waves at certain points. At other points again the two sets of waves weaken one another. In front of the plane wall one can recognise with the rectilinear conductor very distinct maxima and minima, and with the circular conductor the characteristic interference-phenomena of stationary waves which I have described in an earlier paper. I was able to distinguish four nodal points, which were situated at the wall and at 33, 65, and 98 cm. distance from it. We thus get 33 cm. as a closer approximation to the half wave-length of the waves used, and 1·1 thousand-millionth of a second as their period of oscillation, assuming that they travel with the velocity of light. In wires the oscillation gave a wave-length of 29 cm. Hence it appears that these short waves also have a somewhat lower velocity in wires than in air; but the ratio of the two velocities comes very near to the theoretical value —unity—and does not differ from it so much as appeared to

be probable from our experiments on longer waves. This remarkable phenomenon still needs elucidation. Inasmuch as the phenomena are only exhibited in the neighbourhood of the optic axis of the mirror, we may speak of the result produced as an electric ray proceeding from the concave mirror.

I now constructed a second mirror, exactly similar to the first, and attached the rectilinear secondary conductor to it in such a way that the two wires of 50 cm. length lay in the focal line, and the two wires connected to the spark-gap passed directly through the walls of the mirror without touching it. The spark-gap was thus situated directly behind the mirror, and the observer could adjust and examine it without obstructing the course of the waves. I expected to find that, on intercepting the ray with this apparatus, I should be able to observe it at even greater distances; and the event proved that I was not mistaken. In the rooms at my disposal I could now perceive the sparks from one end to the other. The greatest distance to which I was able, by availing myself of a doorway, to follow the ray was 16 metres; but according to the results of the reflection-experiments (to be presently described), there can be no doubt that sparks could be obtained at any rate up to 20 metres in open spaces. For the remaining experiments such great distances are not necessary, and it is convenient that the sparking in the secondary conductor should not be too feeble; for most of the experiments a distance of 6-10 metres is most suitable. We shall now describe the simple phenomena which can be exhibited with the ray without difficulty. When the contrary is not expressly stated, it is to be assumed that the focal lines of both mirrors are vertical.

Rectilinear Propagation

If a screen of sheet zinc 2 metres high and 1 metre broad is placed on the straight line joining both mirrors, and at right angles to the direction of the ray, the secondary sparks disappear completely. An equally complete shadow is thrown by a screen of tinfoil or gold-paper. If an assistant walks across the path of the ray, the secondary spark-gap becomes dark as soon as he intercepts the ray, and again lights up when he leaves the path clear. Insulators do not stop the ray—it

passes right through a wooden partition or door; and it is not without astonishment that one sees the sparks appear inside a closed room. If two conducting screens, 2 metres high and 1 metre broad, are set up symmetrically on the right and left of the ray, and perpendicular to it, they do not interfere at all with the secondary spark so long as the width of the opening between them is not less than the aperture of the mirrors, viz. 1·2 metre. If the opening is made narrower the sparks become weaker, and disappear when the width of the opening is reduced below 0·5 metre. The sparks also disappear if the opening is left with a breadth of 1·2 metre, but is shifted to one side of the straight line joining the mirrors. If the optical axis of the mirror containing the oscillator is rotated to the right or left about 10° out of the proper position, the secondary sparks become weak, and a rotation through 15° causes them to disappear.

There is no sharp geometrical limit to either the ray or the shadows; it is easy to produce phenomena corresponding to diffraction.[1] As yet, however, I have not succeeded in observing maxima and minima at the edge of the shadows.

Polarisation

From the mode in which our ray was produced we can have no doubt whatever that it consists of transverse vibrations and is plane-polarised in the optical sense. We can also prove by experiment that this is the case. If the receiving mirror be rotated about the ray as axis until its focal line, and therefore the secondary conductor also, lies in a horizontal plane, the secondary sparks become more and more feeble, and when the two focal lines are at right angles, no sparks whatever are obtained even if the mirrors are moved close up to one another. The two mirrors behave like the polariser and analyser of a polarisation apparatus.

I next had made an octagonal frame, 2 metres high and 2 metres broad; across this were stretched copper wires 1 mm. thick, the wires being parallel to each other and 3 cm. apart. If the two mirrors were now set up with their focal lines parallel, and the wire screen was interposed perpendicularly to

[1] [See Note 25 at end of book.]

the ray and so that the direction of the wires was perpendicular to the direction of the focal lines, the screen practically did not interfere at all with the secondary sparks. But if the screen was set up in such a way that its wires were parallel to the focal lines, it stopped the ray completely. With regard, then, to transmitted energy the screen behaves towards our ray just as a tourmaline plate behaves towards a plane-polarised ray of light. The receiving mirror was now placed once more so that its focal line was horizontal; under these circumstances, as already mentioned, no sparks appeared. Nor were any sparks produced when the screen was interposed in the path of the ray, so long as the wires in the screen were either horizontal or vertical. But if the frame was set up in such a position that the wires were inclined at 45° to the horizontal on either side, then the interposition of the screen immediately produced sparks in the secondary spark-gap. Clearly the screen resolves the advancing oscillation into two components and transmits only that component which is perpendicular to the direction of its wires. This component is inclined at 45° to the focal line of the second mirror, and may thus, after being again resolved by the mirror, act upon the secondary conductor. The phenomenon is exactly analogous to the brightening of the dark field of two crossed Nicols by the interposition of a crystalline plate in a suitable position.

With regard to the polarisation it may be further observed that, with the means employed in the present investigation, we are only able to recognise the electric force. When the primary oscillator is in a vertical position the oscillations of this force undoubtedly take place in the vertical plane through the ray, and are absent in the horizontal plane. But the results of experiments with slowly alternating currents leave no room for doubt that the electric oscillations are accompanied by oscillations of magnetic force which take place in the horizontal plane through the ray and are zero in the vertical plane. Hence the polarisation of the ray does not so much consist in the occurrence of oscillations in the vertical plane, but rather in the fact that the oscillations in the vertical plane are of an electrical nature, while those in the horizontal plane are of a magnetic nature. Obviously, then, the question, in which of the two planes the oscillation in our ray occurs, cannot be answered

unless one specifies whether the question relates to the electric or the magnetic oscillation. It was Herr Kolaček[1] who first pointed out clearly that this consideration is the reason why an old optical dispute has never been decided.

Reflection

We have already proved the reflection of the waves from conducting surfaces by the interference between the reflected and the advancing waves, and have also made use of the reflection in the construction of our concave mirrors. But now we are able to go further and to separate the two systems of waves from one another. I first placed both mirrors in a large room side by side, with their apertures facing in the same direction, and their axes converging to a point about 3 metres off. The spark-gap of the receiving mirror naturally remained dark. I next set up a plane vertical wall made of thin sheet zinc, 2 metres high and 2 metres broad, at the point of intersection of the axes, and adjusted it so that it was equally inclined to both. I obtained a vigorous stream of sparks arising from the reflection of the ray by the wall. The sparking ceased as soon as the wall was rotated around a vertical axis through about 15° on either side of the correct position; from this it follows that the reflection is regular, not diffuse. When the wall was moved away from the mirrors, the axes of the latter being still kept converging towards the wall, the sparking diminished very slowly. I could still recognise sparks when the wall was 10 metres away from the mirrors, *i.e.* when the waves had to traverse a distance of 20 metres. This arrangement might be adopted with advantage for the purpose of comparing the rate of propagation through air with other and slower rates of propagation, *e.g.* through cables.

In order to produce reflection of the ray at angles of incidence greater than zero, I allowed the ray to pass parallel to the wall of the room in which there was a doorway. In the neighbouring room to which this door led I set up the receiving mirror so that its optic axis passed centrally through the door and intersected the direction of the ray at right angles. If the plane conducting surface was now set up vertically at

[1] [F. Kolaček, *Wied. Ann.* **34**, p. 676, 1888.]

the point of intersection, and adjusted so as to make angles of 45° with the ray and also with the axis of the receiving mirror, there appeared in the secondary conductor a stream of sparks which was not interrupted by closing the door. When I turned the reflecting surface about 10° out of the correct position the sparks disappeared. Thus the reflection is regular, and the angles of incidence and reflection are equal. That the action proceeded from the source of disturbance to the plane mirror, and hence to the secondary conductor, could also be shown by placing shadow-giving screens at different points of this path. The secondary sparks then always ceased immediately; whereas no effect was produced when the screen was placed anywhere else in the room. With the aid of the circular secondary conductor it is possible to determine the position of the wave-front in the ray; this was found to be at right angles to the ray before and after reflection, so that in the reflection it was turned through 90°.

Hitherto the focal lines of the concave mirrors were vertical, and the plane of oscillation was therefore perpendicular to the plane of incidence. In order to produce reflection with the oscillations in the plane of incidence, I placed both mirrors with their focal lines horizontal. I observed the same phenomena as in the previous position; and, moreover, I was not able to recognise any difference in the intensity of the reflected ray in the two cases. On the other hand, if the focal line of the one mirror is vertical, and of the other horizontal, no secondary sparks can be observed. The inclination of the plane of oscillation to the plane of incidence is therefore not altered by reflection, provided this inclination has one of the two special values referred to; but in general this statement cannot hold good. It is even questionable whether the ray after reflection continues to be plane-polarised. The interferences which are produced in front of the mirror by the intersecting wave-system, and which, as I have remarked, give rise to characteristic phenomena in the circular conductor, are most likely to throw light upon all problems relating to the change of phase and amplitude produced by reflection.

One further experiment on reflection from an electrically eolotropic surface may be mentioned. The two concave mirrors were again placed side by side, as in the reflection-experiment

first described; but now there was placed opposite to them, as a reflecting surface, the screen of parallel copper wires which has already been referred to. It was found that the secondary spark-gap remained dark when the wires intersected the direction of the oscillations at right angles, but that sparking began as soon as the wires coincided with the direction of the oscillations. Hence the analogy between the tourmaline plate and our surface which conducts in one direction is confined to the transmitted part of the ray.[1] The tourmaline plate absorbs the part which is not transmitted; our surface reflects it. If in the experiment last described the two mirrors are placed with their focal lines at right angles, no sparks can be excited in the secondary conductor by reflection from an isotropic screen; but I proved to my satisfaction that sparks are produced when the reflection takes place from the eolotropic wire grating, provided this is adjusted so that the wires are inclined at 45° to the focal lines. The explanation of this follows naturally from what has been already stated.

Refraction

In order to find out whether any refraction of the ray takes place in passing from air into another insulating medium, I had a large prism made of so-called hard pitch, a material like asphalt. The base was an isosceles triangle 1·2 metres in the side, and with a refracting angle of nearly 30°. The refracting edge was placed vertical, and the height of the whole prism was 1·5 metres. But since the prism weighed about 12 cwt., and would have been too heavy to move as a whole, it was built up of three pieces, each 0·5 metre high, placed one above the other. The material was cast in wooden boxes which were left around it, as they did not appear to interfere with its use. The prism was mounted on a support of such height that the middle of its refracting edge was at the same height as the primary and secondary spark-gaps. When I was satisfied that refraction did take place, and had obtained some idea of its amount, I arranged the experiment in the following manner:—The producing mirror was set up at a distance of 2·6 metres from the prism and facing one of the refracting surfaces, so that the axis of the beam was directed as nearly as possible towards the centre

[1] [See Note 26 at end of book.]

of mass of the prism, and met the refracting surface at an angle of incidence of 25° (on the side of the normal towards the base). Near the refracting edge and also at the opposite side of the prism were placed two conducting screens which prevented the ray from passing by any other path than that through the prism. On the side of the emerging ray there was marked upon the floor a circle of 2·5 metres radius, having as its centre the centre of mass of the lower end of the prism. Along this the receiving mirror was now moved about, its aperture being always directed towards the centre of the circle. No sparks were obtained when the mirror was placed in the direction of the incident ray produced; in this direction the prism threw a complete shadow. But sparks appeared when the mirror was moved towards the base of the prism, beginning when the angular deviation from the first position was about 11°. The sparking increased in intensity until the deviation amounted to about 22°, and then again decreased. The last sparks were observed with a deviation of about 34°. When the mirror was placed in a position of maximum effect, and then moved away from the prism along the radius of the circle, the sparks could be traced up to a distance of 5-6 metres. When an assistant stood either in front of the prism or behind it the sparking invariably ceased, which shows that the action reaches the secondary conductor through the prism and not in any other way. The experiments were repeated after placing both mirrors with their focal lines horizontal, but without altering the position of the prism. This made no difference in the phenomena observed. A refracting angle of 30° and a deviation of 22° in the neighbourhood of the minimum deviation corresponds to a refractive index of 1·69. The refractive index of pitch-like materials for light is given as being between 1·5 and 1·6. We must not attribute any importance to the magnitude or even the sense of this difference,[1] seeing that our method was not an accurate one, and that the material used was impure.

We have applied the term rays of electric force to the phenomena which we have investigated. We may perhaps further designate them as rays of light of very great wave-length. The experiments described appear to me, at any rate, eminently

[1] [See Note 27 at end of book.]

adapted to remove any doubt as to the identity of light, radiant heat, and electromagnetic wave-motion. I believe that from now on we shall have greater confidence in making use of the advantages which this identity enables us to derive both in the study of optics and of electricity.

Explanation of the Figures.—In order to facilitate the repetition and extension of these experiments, I append in the accompanying Figs. 35, 36_a, and 36_b, illustrations of the apparatus which I used, although these were constructed simply for the purpose of experimenting at the time and without any regard to durability. Fig. 35 shows in plan and elevation (section) the producing mirror. It will be seen that the framework of it consists of two horizontal frames (*a*, *a*) of parabolic form, and four vertical supports (*b*,*b*) which are screwed to each of the frames so as to support and connect them. The sheet metal reflector is clamped between the frames and the supports, and fastened to both by numerous screws. The supports project above and below beyond the sheet metal so that they can be used as handles in handling the mirror. Fig. 36_a represents the primary conductor on a somewhat larger scale. The two metal parts slide with friction in two sleeves of strong paper which are held together by indiarubber bands. The sleeves themselves are fastened by four rods of sealing-wax to a board which again is tied by indiarubber bands to a strip of wood forming part of the frame which can be seen in Fig. 35. The two leading

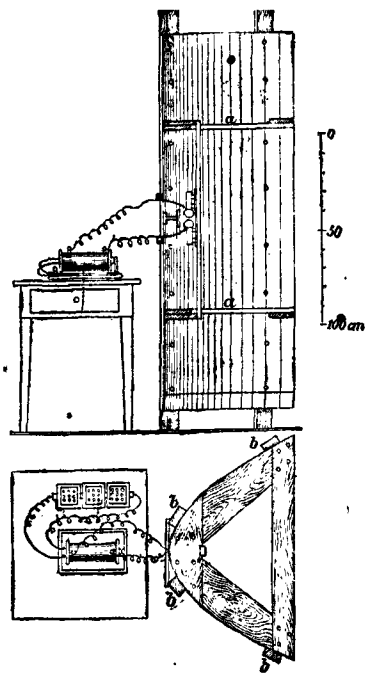


Fig. 35.

wires (covered with gutta-percha) terminate in two holes bored in the knobs of the primary conductor. This arrangement allows of all necessary motion and adjustment of the various parts of the conductor; it can be taken to pieces and put together again in a few minutes, and this is essential in order that the knobs may be frequently repolished. Just at the points where the leading wires pass through the mirror, they

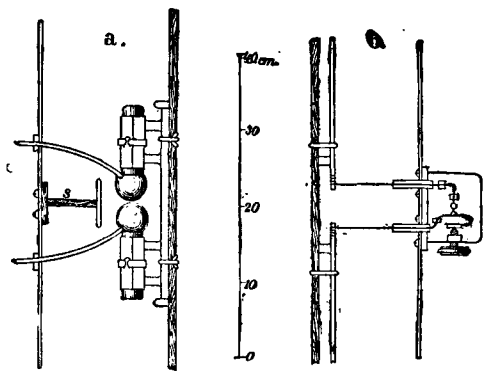


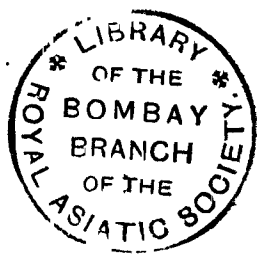
Fig. 36.

are surrounded during the discharge by a bluish light. The smooth wooden screen *s* is introduced for the purpose of shielding the spark-gap from this light, which otherwise would interfere seriously with the production of the oscillations. Lastly, Fig. 36_b represents the secondary spark-gap. Both parts of the secondary conductor are again attached by sealing-wax rods and indiarubber bands to a slip forming part of the wooden framework. From the inner ends of these parts the leading wires, surrounded by glass tubes, can be seen proceeding through the mirror and bending towards one another. The upper wire carries at its pole a small brass knob. To the lower wire is soldered a piece of watch-spring which carries the second pole, consisting of a fine copper point. The point is intentionally chosen of softer metal than the knob; unless this precaution is taken the point easily penetrates into the knob, and the minute sparks disappear from sight in the small hole thus produced. The figure shows how the point is adjusted by a screw which presses against the spring that is insulated from it by a glass plate. The spring is bent in a particular way in order to secure

finer motion of the point than would be possible if the screw alone were used.

No doubt the apparatus here described can be considerably modified without interfering with the success of the experiments. Acting upon friendly advice, I have tried to replace the spark-gap in the secondary conductor by a frog's leg prepared for detecting currents; but this arrangement which is so delicate under other conditions does not seem to be adapted for these purposes.[1]

[1] [See Note 28 at end of book.]



XII

ON THE MECHANICAL ACTION OF ELECTRIC WAVES IN WIRES

(*Wiedmann's Ann.* **42**, p. 407, 1891.)

THE investigation of the mechanical forces to which a conductor is subjected under the action of a series of electric waves appeared to me to be desirable for several reasons. In the first place, these forces might supply a means of investigating such waves quantitatively, provided that the effects observed were of sufficient magnitude and regularity. Hitherto almost the only quantitative determinations have been based on the heating effect of the waves. In the hands of Herren Rubens and Ritter this method has given excellent results;[1] but the observation of the mechanical forces offers in many cases the advantage of simplicity. In the second place, by examining the nature and distribution of the mechanical forces, I hoped to find a means of demonstrating the existence of the magnetic force in addition to the electric force. Only the latter has manifested itself in the observations hitherto made;[2] and as the ordinary methods of detecting magnetic force are of no avail here, it appeared to be worth while trying whether a new method would prove more serviceable. In the third and last place—and this was more especially the object of the investigation—I hoped to be able to devise some way of making observations on waves in free air,—that is to say, in such a manner that any disturbances which might be observed could

[1] H. Rubens and R. Ritter, *Wied. Ann.* **40**, p. 55, 1890.

[2] If I have myself on former occasions happened to speak of the observation of nodes of the magnetic waves, this mode of expression was only justified by theory and not required by experiment.

in no wise be referred to any action-at-a-distance. This last hope was frustrated by the feebleness of the effects produced under the circumstances. I had to content myself with examining the effects produced by waves travelling along wires, although in so doing the most important object of the experiments was missed. The mechanical actions produced by waves in wires may be and will be regarded as being due to attractions caused by the electrification of the wires and by the currents flowing in them. For this reason researches on waves in wires cannot be made use of to decide between the older and the newer views. If, however, we start from the point of view from which waves in wires are regarded simply as a special form of waves travelling in air, it is a matter of indifference whether we make the one form or the other the object of our experiments.

1. *The System of Waves Employed*

After trying several ways of disposing the waves, and after obtaining results which in the main were concordant, I decided to adhere to Herr Lecher's arrangement as being the neatest and the most suitable for the investigation.[1] Fig. 37 shows the form thereof.

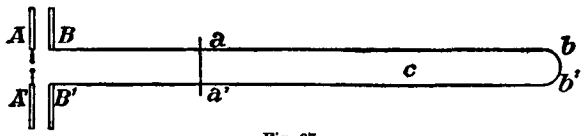


Fig. 37.

$A\,A'$ is the same conductor which was always used before as the primary conductor, and consists of two square plates, each 40 cm. in the side, connected by a wire 60 cm. long which contains a 2 mm. spark-gap. A small induction-coil was used as an exciter; this was supplied with current from two accumulators, and its maximum spark-length was only 4 cm. Single discharges of this smaller apparatus were certainly less efficient than those of a larger induction-coil, but this

[1] E. Lecher, *Wied. Ann.* **41**, p. 850, 1890.

drawback was more than compensated for by the more rapid succession of discharges. Opposite the plates A and A', and at a distance of 10 cm. from them, stood the plates B and B', from which two parallel wires, about 30 cm. apart, are led to a distance of 6·8 metres, and there are connected together between b and b'. At a variable distance $a\,a'$ from their origin these wires are placed in communication with each other by means of a second connection or *bridge*. When this bridge is in a certain position, at a distance of about 1·2 metre from $B\,B'$, there takes place in the interval between $a\,a'$ and $b\,b'$ a very energetic oscillation. This indicates the half wave-length of a stationary wave, and, as Herr Lecher has shown, it is produced by resonance between this oscillation itself and the primary oscillation, which here takes place in the interval between $A\,A'$ on the one hand, and $B\,a\;a'B'$ on the other hand. Any shifting of the bridge increases one of the two periods of oscillation, and at the same time diminishes the other; hence the peculiar definiteness of adjustment with this arrangement. Besides its general excellence it offers for our present purpose several special advantages. Since the forces to be observed are very small, we have to protect carefully the conductors which are subjected to them from external electrostatic effects. With the arrangement here used this is possible, because the wires, which we must necessarily place near the test-body, form a connected conducting system. If in our experiments we surround the working parts (of the apparatus) with a wire network, and connect this with the nodal points at $a\,a'$ and $b\,b'$, the protection is made complete without interfering with the vibration. Hence the experiments are carried out in this way. Again, since the conductors which are to be subjected to the forces do not, like the resonators previously used, pick out a definite vibration from the whole disturbance, we could only expect confused results if we did not otherwise take care to produce a simple oscillation of definite wave-length and with nodes in known positions. This condition is fulfilled in the above arrangement; for there can be no doubt that the points $a\,a'$ and $b\,b'$ are nodal points of all oscillations excited between them, and that among these only the longest oscillation, strengthened by resonance, rises to a considerable magnitude. Clearly, we do not narrow the scope of the experiments by

contenting ourselves with the investigation of half a wave-length. Finally, the conditions of our oscillation are practically the same whether the two wires are stretched straight, or whether they are bent side by side in any desired way; just as, in the case of acoustic vibrations of air in tubes, it is not of much importance whether the tubes are straight or crooked. We can thus easily bring our oscillation into all possible positions with respect to the test-body which is held in a fixed position. As a matter of fact, the various relative positions were always obtained by shifting the wire, even in cases in which it may appear from the text that the test-body had been shifted.

2. *The Electric Force*

For the purpose of measuring the mechanical action of the electric force, I made use of a small cylindrical tube of gold paper 5·5 cm. long, and 0·7 cm. in diameter. This was suspended by a silk fibre with its axis horizontal; a very small magnet gave the tube a definite position of rest, and a deviation from this position was measured by means of a small mirror. The whole system hung in a glass case, as shown in Fig. 38. When the apparatus was subjected to the action of the oscillation, the needle tended to set along the mean direction of the electric force, and was thus deflected from the position of rest. In order to increase these deflections I brought the two wires in the neighbourhood of the apparatus nearer to one another and to the test-body—in fact, within a few centimetres; and in order to strengthen the action I attached small plates to the wires opposite to the ends of the test-body, as shown for one special case in the figure. Under

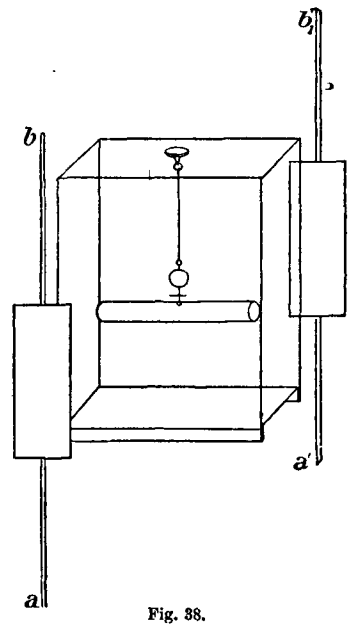


Fig. 38.

these circumstances first deflections of 100 scale divisions and above could be obtained. These first throws exhibited a satisfactory regularity; when the same experiment was repeated several times, the separate results only differed from one another by a few per cent. The differences between single discharges ought to be much greater, but the throw of the needle gives the mean effect of very many discharges. In order to show how these throws can be used in quantitative experiments I here quote two series of observations. The first of these is intended to illustrate the effect of resonance. The apparatus was set up at c at the antinode of an oscillation, and the wires $a\,b$ and $a'\,b'$ were brought near to it, as shown in Fig. 38. The bridge $a\,a'$ was now placed at various distances e from the origin $B\,B'$ of the wires, the induction-apparatus was put into action, and the magnitude i of the throw measured. The respective values of e and i in the neighbourhood of the maximum were:—

$e =$	80	90	100	110	120	130	140	150	160 cm.
$i =$	5·3	10·0	21·8	51·2	44·1	19·3	10·3	5·7	4·2 div.

When the throws are represented graphically it is seen that their course is regular and exhibits a pronounced maximum between 110 and 120 cm. In fact, the throws reach their largest value $i = 60{\cdot}6$ scale divisions at $e = 114$ cm.

The second series of observations was intended to exhibit the decrease in the intensity of the oscillation from the antinode c to the node b. For this purpose the distance was divided into 12 equal divisions, and the apparatus was introduced at the 13 end-points. The following first throws i were obtained:—

1	2	3	4	5	6	7	8	9	10	11	12	13
80·5	80·5	79·0	77·0	65·6	57·8	50·0	38·5	27·5	17·5	7·0	1·0	0

These values again give a sufficiently smooth curve and enable us to form an idea of the nature of the oscillation, and to convince ourselves that it differs appreciably from the simple sine-oscillation.

Other experiments which I planned had reference to the direction of the electric force in the neighbourhood of the wires. These experiments gave no fresh information beyond

what might be regarded as already settled. In the interval between the wires the needle tended to set along the shortest line between the two wires; outside this space it tended to take up the direction towards the nearest wire. Thus there was always an apparent attraction to be observed between the ends of the tube and the nearest parts of the wires.

3. *The Magnetic Force*

In order to investigate the magnetic force I made use of a circular hoop of aluminium wire. The diameter of this hoop was 65 mm., and that of the wire was 2 mm. The hoop was suspended so that it could turn about a vertical diameter, and, like the cylinder in the last section, was provided with a magnet, mirror, and glass case. Fig. 39 gives a sketch of the apparatus used.

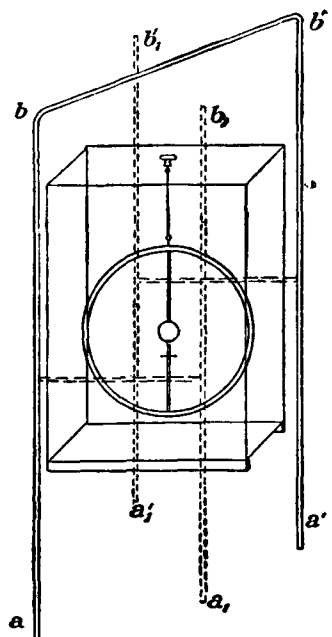


Fig. 39.

If we disregard for a moment our knowledge of the magnetic force we should expect that, under the influence of the oscillation, the hoop would behave just like the cylinder, and therefore that the direction of the parts which are farthest from the axis of rotation, *i.e.* that the horizontal diameter of the hoop would play the same part as the axis of length of the cylinder. We should therefore expect the end-points of the horizontal diameter would everywhere be attracted by the nearest parts of the wires through which the waves are passing, and that this action would be strongest at the antinode of the oscillation, and would cease in the neighbourhood of the nodes where the electric force itself disappears. But if we actually hang up the hoop at the node $b\ b'$, in the manner shown in Fig. 39, we observe other and unexpected phenomena. In the

first place, the ring does not remain at rest under the influence of the oscillation, but exhibits deflections of the same order of magnitude as those shown by the cylinder at the antinode of the oscillation. In the second place, the deflection does not indicate an attraction but a repulsion between the neighbouring points of the hoop and the wires. That the repulsion is a consequence of the oscillation itself is shown by the fact that its magnitude is found to be determined by resonance, according to the same law as that of the electrical action. If we leave the hoop inside the bent wire $b\,b'$, but alter the relative positions of the two, w find that the horizontal diameter always, and from all sides, endeavours to take up a position perpendicular to the plane of the bent wire.

After these experiments alone, and apart from any knowledge obtained otherwise, we may therefore assert that, in addition to the electric oscillation, there is present an oscillation of another kind whose nodal points do not coincide with those of the electric oscillation, and that this oscillation, like the electrical one, exhibits itself as a directive change of space-conditions, but that the characteristic direction of the new oscillation is perpendicular to the electrical one.

We may indeed, going beyond mere observation, at once identify the new oscillation with the magnetic oscillation required by theory. The rapidly alternating magnetic force must induce in the closed hoop a current alternating rhythmically with it, and the reaction between these causes the deflection of the loop. The magnetic force has its maximum value at the nodes of the electric oscillation, and just there its direction is perpendicular to the plane of the bent wire. We can most easily understand the repulsion between the fixed wires and the neighbouring parts of the hoop by regarding it as the effect of currents flowing along these paths. The current deduced in the hoop must continually annul the effect of the inducing current in the interior of the hoop; it must therefore at every instant be in the opposite direction to the latter, and must accordingly be repelled by it.

All the remaining phenomena of disturbance which are observed with the suspended hoop, can without difficulty be connected with the above explanation. Under certain circumstances complications arise. For example, if we leave the

arrangement in the state shown in Fig. 39, but move the hoop from the node $b\,b'$ towards the antinode of the oscillation, the repulsion rapidly diminishes; at a certain distance it becomes zero, and then changes into an attraction which increases until we arrive at the antinode. In one special case, for example, the repulsion at $b\,b'$ amounted to 20 scale divisions, disappeared at a distance of 95 cm. from the end, and then changed into an attraction of which the maximum value was measured by 44 scale divisions. Clearly these changes are not to be explained by the behaviour of the magnetic force alone, but by the joint action of the magnetic with the electric force; of these the latter preponderates considerably at c, the former at $b\,b'$. By eliminating the electric force we can confirm this view and follow the course of the magnetic oscillation. For this purpose we set up two other wires parallel to the wires $a\,b$ and $a'\,b'$, but only 20 cm. long, and in such a position that they are symmetrical towards the wires $a\,b$ and $a'\,b'$ with reference to the position of rest of the hoop, as shown by the dotted lines $a_1\,b_1$ and $a_1'\,b_1'$ in Fig. 40. We connect $a\,b$ with $a_1\,b_1$ and $a'\,b'$ with $a_1'\,b_1'$. Clearly this almost annuls the electric action, but scarcely affects the magnetic. In fact, we now observe that at all distances the movable ring is repelled from the fixed wires. This repulsion diminishes continuously from the ends towards the middle of the oscillation; it there reaches a minimum which, in the particular instance referred to, amounted to 4 scale divisions. If the electric oscillation were a real sine-oscillation, the magnetic force would necessarily vanish at its antinode; but we saw at once, from the distribution of the electric force, that this simple assumption did not hold good, and so we can easily understand the existence of a residual magnetic force at the antinode of the oscillation.

As required by theory, the mechanical effects of the electric and of the magnetic force prove to be, in general, of the same order of magnitude; the preponderance of the one over the other in each particular case is mainly determined by the proportions of the neighbouring parts of the ring and of the fixed conductors. The more these approximate to the state of infinitely thin wires, the more the magnetic force comes into prominence; the broader the surfaces which are attached to them, the more is the magnetic force overpowered by the

electric force. It is evident, even from the simple examples of forms of conductor which we have chosen for the detailed investigation, that a conductor of any form whatever inside a train of electromagnetic waves must be subjected to the action of forces which are complicated and not always easy to understand.

XIII

ON THE FUNDAMENTAL EQUATIONS OF ELECTRO-MAGNETICS FOR BODIES AT REST

(*Göttinger Nachr.* March 19, 1890; *Wiedemann's Ann.* **40**, p. 577.)

THE system of ideas and formulæ by which Maxwell represented electromagnetic phenomena is in its possible developments richer and more comprehensive than any other of the systems which have been devised for the same purpose. It is certainly desirable that a system which is so perfect, as far as its contents are concerned, should also be perfected as far as possible in regard to its form. The system should be so constructed as to allow its logical foundations to be easily recognised; all unessential ideas should be removed from it, and the relations of the essential ideas should be reduced to their simplest form. In this respect Maxwell's own representation does not indicate the highest attainable goal; it frequently wavers between the conceptions which Maxwell found in existence, and those at which he arrived. Maxwell starts with the assumption of direct actions-at-a-distance; he investigates the laws according to which hypothetical polarisations of the dielectric ether vary under the influence of such distance-forces; and he ends by asserting that these polarisations do really vary thus, but without being actually caused to do so by distance-forces.[1] This procedure leaves behind it the unsatisfactory feeling that there must be something wrong about either the final result or the way which led to it. Another effect of this procedure is that in the formulæ there are retained a number of superfluous,

[1] The same remark applies to v. Helmholtz's paper in vol. **72** of *Crelle's Journal*,—not, indeed, throughout, but as far as relates to the special values of the constants, which allow the distance-forces to vanish from the final results, and which, therefore, lead to the theory here supported.

and in a sense rudimentary, ideas which only possessed their proper significance in the older theory of direct action-at-a-distance. Among such rudimentary ideas of a physical nature I may mention that of dielectric displacement in free ether, as distinguished from the electric force which produces it and the relation between the two—the specific inductive capacity of the ether. These distinctions have a meaning so long as we can remove the ether from a space and yet allow the force to persist in it. This was conceivable, according to the conception from which Maxwell started; it is not conceivable, according to the conception to which we have been led by his researches. As a rudimentary idea of a mathematical nature I may mention the predominance of the vector-potential in the fundamental equations. In the construction of the new theory the potential served as a scaffolding; by its introduction the distance-forces which appeared discontinuously at particular points were replaced by magnitudes which at every point in space were determined only by the conditions at the neighbouring points. But after we have learnt to regard the forces themselves as magnitudes of the latter kind, there is no object in replacing them by potentials unless a mathematical advantage is thereby gained. And it does not appear to me that any such advantage is attained by the introduction of the vector-potential in the fundamental equations; furthermore, one would expect to find in these equations relations between the physical magnitudes which are actually observed, and not between magnitudes which serve for calculation only.

Again, the incompleteness of form referred to renders it more difficult to apply Maxwell's theory to special cases. In connection with such applications I have been led to endeavour for some time past to sift Maxwell's formulæ and to separate their essential significance from the particular form in which they first happened to appear. The results at which I have arrived are set forth in the present paper. Mr. Oliver Heaviside has been working in the same direction ever since 1885. From Maxwell's equations he removes the same symbols as myself; and the simplest form which these equations[1]

[1] These equations will be found in the *Phil. Mag.* for February 1888. Reference is there made to earlier papers in the *Electrician* for 1885, but this source was not accessible to me.

thereby attain is essentially the same as that at which I arrive. In this respect, then, Mr. Heaviside has the priority. Nevertheless, I hope that the following representation will not be deemed superfluous. It does not claim to set forth matters in a final form; but only in such a manner as to admit of further improvements more easily than has hitherto been possible.

I divide the subject into two parts. In the first part (A) I give the fundamental ideas and the formulæ by which they are connected. Explanations will be added to the formulæ; but these explanations are not to be regarded as proofs of the formulæ. The statements will rather be given as facts derived from experience; and experience must be regarded as their proof. It is true, meanwhile, that each separate formula cannot be specially tested by experience, but only the system as a whole. But practically the same holds good for the system of equations of ordinary dynamics.

In the second part (B) I state in what manner the facts which are directly observed can be systematically deduced from the formulæ; and, hence, by what experiences the correctness of the system can be proved. A complete treatment of this part would naturally assume very large dimensions, and therefore mere indications must here suffice.

A. The Fundamental Ideas and their Connection

1. *Electric and Magnetic Force*

Starting from rest, the interior of all bodies, including the free ether, can experience disturbances which we denote as electrical, and others which we denote as magnetic. The nature of these changes of state we do not know, but only the phenomena which their presence causes. Regarding these latter as known we can, with their aid, determine the geometrical relations of the changes of state themselves. The disturbances of the electric and the magnetic kind are so connected with one another that disturbances of the one kind can continuously exist independently of those of the other kind; but that, on the other hand, it is not possible for disturbances of either of the two kinds to experience temporary fluctuations without exciting simultaneously disturbances of the other kind.

The production of the change of state necessitates an expenditure of energy; this energy is again released when the disturbance disappears; hence the presence of the disturbance represents a stock of energy. At any given point the changes of state of either kind can be distinguished as to direction, sense, and magnitude. For the determination, therefore, of the electrical as well as of the magnetic state, it is necessary to specify a directed magnitude or the three components thereof. But it is an essential and important hypothesis of our present theory that the specification of a single directed magnitude is sufficient to determine completely the change of state under consideration. Certain phenomena, *e.g.* those of permanent magnetism, dispersion, etc., are not intelligible from this standpoint; they require that the electric or magnetic conditions of any point should be represented by more than one variable.[1] For this very reason such phenomena are excluded from our considerations in their present state.

That directed magnitude by means of which we first determine the electrical state, we call the electric force. The phenomenon by which we define the electric force is the mechanical force which a certain electrified body experiences in empty space under electrical stress. That is to say, for empty space we make the component of the electric force in any given direction proportional to the component of this mechanical force in the same direction. By electric force at a point in a ponderable body we understand the electric force at this point inside an infinitely small cylindrical space, infinitely narrow as compared with its length, bored out of the body in such a way that its direction coincides with that of the force—a requirement which, as experience shows, can always be satisfied. And whatever may be the relation between the force so measured and the actual change of state of the body, it certainly must, in accordance with our hypothesis, determine the change of state without ambiguity. If we everywhere replace the word "electric" by the word "magnetic," and the electrified test-body by the pole of a magnetic needle, we obtain the definition of magnetic force. In order to settle the sense of both forces in the conventional manner, let us further stipulate that the electrified test-body is charged with vitreous electricity, and

[1] [See Note 29 at end of book.]

that the pole of the magnetic needle used is the one which points towards the north. The units of the forces are still reserved. The components of the electric force in the directions x, y, z, we shall denote as X, Y, Z, and the corresponding components of the magnetic force as L, M, N.

2. *The Energy of the Field*

The stock of electrical energy in a portion of a body, within which the electric force has a constant value, is a homogeneous quadratic function of the three components of the electric force. The corresponding statement holds good for the supply of magnetic energy. The total supply of energy we shall denote as the electromagnetic; it is the sum of the electrical and the magnetic.

According to this, the amount of energy of either kind per unit volume is for an isotropic body equal to the product of the square of the total force under consideration and a constant. The magnitude of the latter may be different for the electric and the magnetic energy; it depends upon the material of the body and the choice of the units for energy and for the forces. We shall measure the energy in absolute Gauss's measure; and shall now fix the units of the forces by stipulating that in free ether the value of the constants shall be equal to $1/8\pi$, so that the energy of unit volume of the stressed ether will be

$$\frac{1}{8\pi}(X^2 + Y^2 + Z^2) + \frac{1}{8\pi}(L^2 + M^2 + N^2).$$

When the forces are measured in this manner, we say that they are measured in absolute Gauss's units.[1] The dimensions of the electric force become the same as those of the magnetic force. Both are such that their square has the dimensions of energy per unit volume; or, expressed in the usual notation, the dimensions of both are $M^{1/2}L^{-1/2}T^{-1}$.

For every isotropic ponderable body we can now, in accordance with what has been stated, put the energy per unit volume as equal to

$$\frac{\epsilon}{8\pi}(X^2 + Y^2 + Z^2) + \frac{\mu}{8\pi}(L^2 + M^2 + N^2).$$

[1] See H. Helmholtz, *Wied. Ann.* **17**, p. 42, 1882.

The new constants (ϵ and μ) here introduced are necessarily positive, and are simply numbers. We shall call ϵ the specific inductive capacity (*Dielektricitätsconstante*) and μ the magnetic permeability (*Magnetisirungsconstante*) of the substance. Clearly ϵ and μ are numerical ratios, by means of which we compare the energy of one material with the energy of another material. A definite value of either does not follow simply from the nature of a single substance itself. This is what we mean when we say that the specific inductive capacity and the magnetic permeability are not intrinsic constants of a substance. There is nothing wrong in saying that these constants are equal to unity for the ether; but this does not state any fact derived from experience; it is only an arbitrary stipulation on our part.

For crystalline bodies the energy per unit of volume will be equal to

$$\frac{1}{8\pi}(\epsilon_{11}X^2+\epsilon_{22}Y^2+\epsilon_{33}Z^2+2\epsilon_{12}XY+2\epsilon_{23}YZ+2\epsilon_{13}XZ)$$

$$+\frac{1}{8\pi}(\mu_{11}L^2+\mu_{22}M^2+\mu_{33}N^2+2\mu_{12}LM+2\mu_{23}MN+2\mu_{13}LN).$$

By a suitable choice of axes either the one part or the other of this expression can be transformed into a sum of three squares. It is even probable that the same choice of axes would thus transform both parts. The ϵ and μ must be such that in the transformation into a sum of squares all the coefficients would become positive.

3. *Connection of the Forces in the Ether*

We assume that the system of co-ordinates is such that the direction of positive x is straight out in front of us, the direction of positive z upwards, and that y increases from left to right.[1] Assuming this, the electric and magnetic forces in the ether are connected with each other according to the following equations:—

[1] Unfortunately for the English reader this is not the system employed by Maxwell, but the symmetric one. Hence follow some differences from Maxwell's formulæ as to the signs + and −. The system is that which is employed in v. Helmholtz's papers.

$$(3_a)\quad \begin{cases} A\dfrac{dL}{dt} = \dfrac{dZ}{dy} - \dfrac{dY}{dz}, \\ A\dfrac{dM}{dt} = \dfrac{dX}{dz} - \dfrac{dZ}{dx}, \\ A\dfrac{dN}{dt} = \dfrac{dY}{dx} - \dfrac{dX}{dy}; \end{cases} \qquad (3_b)\quad \begin{cases} A\dfrac{dX}{dt} = \dfrac{dM}{dz} - \dfrac{dN}{dy}, \\ A\dfrac{dY}{dt} = \dfrac{dN}{dx} - \dfrac{dL}{dz}, \\ A\dfrac{dZ}{dt} = \dfrac{dL}{dy} - \dfrac{dM}{dx}, \end{cases}$$

in addition to which we have the equations (which are not inconsistent with the above)—

$$(3_c)\quad \frac{dL}{dx} + \frac{dM}{dy} + \frac{dN}{dx} = 0, \qquad \frac{dX}{dx} + \frac{dY}{dy} + \frac{dZ}{dz} = 0$$

as a supplement distinguishing the ether from ponderable matter.

After these equations are once found, it no longer appears expedient to deduce them (in accordance with the historical course) from conjectures as to the electric and magnetic constitution of the ether and the nature of the acting forces,—all these things being entirely unknown. Rather is it expedient to start from these equations in search of such further conjectures respecting the constitution of the ether.

Since the dimensions of X, Y, Z, and of L, M, N are the same, the constant A must be the reciprocal of a velocity. It is in reality an intrinsic constant of the ether; in saying this we assert that its magnitude is independent of the presence of any other body, or of any arbitrary stipulations on our part.

We multiply all our equations by $(1/4\pi A).d\tau$; further multiply the members of the series separately by L, M, N, X, Y, Z respectively, and add all together. We integrate both sides of the resulting equation over any definite space, of which the element of surface $d\omega$ makes, with the co-ordinate axes, the angle n,x, n,y, n,z. The integration can be carried out on the right-hand side of the equation, and we get—

$$\frac{d}{dt}\int\left\{\frac{1}{8\pi}(X^2+Y^2+Z^2)+\frac{1}{8\pi}(L^2+M^2+N^2)\right\}d\tau$$
$$=\frac{1}{4\pi A}\int\{(NY-MZ)\cos n,x+(LZ-NX)\cos n,y$$
$$+(MX-LY)\cos n,z\}d\omega.$$

The integral on the left-hand side is the electromagnetic energy of the space: the equation, therefore, gives us the variation of this energy, expressed in magnitudes which relate only to the bounding surface of the space.

4. *Isotropic Non-Conductors*

In homogeneous isotropic non-conductors the phenomena are qualitatively identical with those in free ether. Quantitatively they differ in two respects: in the first place, the intrinsic constant has a value different from what it has in the ether; and in the second place, the expression for the energy per unit volume contains, as already explained, the constants ϵ and μ. The following equations represent these statements, and are in accord with experience:—

$$(4_a)\quad \begin{cases} A\mu\dfrac{dL}{dt} = \dfrac{dZ}{dy} - \dfrac{dY}{dz}, \\ A\mu\dfrac{dM}{dt} = \dfrac{dX}{dz} - \dfrac{dZ}{dx}, \\ A\mu\dfrac{dN}{dt} = \dfrac{dY}{dz} - \dfrac{dX}{dy}; \end{cases} \qquad (4_b)\quad \begin{cases} A\epsilon\dfrac{dX}{dt} = \dfrac{dM}{dz} - \dfrac{dN}{dy}, \\ A\epsilon\dfrac{dY}{dt} = \dfrac{dN}{dx} - \dfrac{dL}{dz}, \\ A\epsilon\dfrac{dZ}{dt} = \dfrac{dL}{dy} - \dfrac{dM}{dx}. \end{cases}$$

For if, for a moment, we determine the measure of the forces in the non-conductor as we have previously done in the ether, and accordingly replace X, Y, Z by $X/\sqrt{\epsilon}$, $Y/\sqrt{\epsilon}$, $Z/\sqrt{\epsilon}$, and L, M, N by $L/\sqrt{\mu}$, $M/\sqrt{\mu}$, $N/\sqrt{\mu}$; then the equations assume exactly the form of the equations for the ether—with this single difference, that the magnitude A is replaced by the magnitude $A/\sqrt{\epsilon\mu}$. If we retain, on the other side, our measure of the forces, we can consistently assign to the energy the requisite value. For by carrying out the same operations which we employed in the preceding section, we get—

$$\frac{d}{dt}\int\left\{\frac{\epsilon}{8\pi}(X^2+Y^2+Z^2)+\frac{\mu}{8\pi}(L^2+M^2+N^2)\right\}d\tau$$
$$=\frac{1}{4\pi A}\int\{(NY-MZ)\cos n,x+(LZ-NX)\cos n,y$$
$$+(MX-LY)\cos n,z\}\,d\omega.$$

The general statements by which we have been guided to equations (4) no longer hold good when the non-conductor is not homogeneous. The question therefore arises—Do our equations hold good in this case? Experience answers this question in the affirmative; we may therefore regard the magnitudes ϵ and μ in equations (4_a) and (4_b) as variable from point to point.

5. *Crystalline Non-Conductors*

We can obtain a representation of the processes that take place in such bodies—whose structure differs in different directions, but whose electromagnetic properties merge into those of isotropic non-conductors as the eolotropy disappears—by regarding the time-variations of the forces on the left hand of our equations as perfectly general linear functions of the space-variations of the forces of the opposite kind on the right hand. The generality of form of these linear functions and the choice of their constants is, however, restricted by assuming that the same operation which has already furnished us with an equation for the variation of energy will always do so, and by stipulating that the energy shall thereby be expressed in the form already established. By these considerations we are led to the following equations, which, in fact, suffice for the representation of the most important phenomena:—

$$(5_a)\quad \begin{cases} A\left(\mu_{11}\dfrac{dL}{dt}+\mu_{12}\dfrac{dM}{dt}+\mu_{13}\dfrac{dN}{dt}\right)=\dfrac{dZ}{dy}-\dfrac{dY}{dz}, \\ A\left(\mu_{12}\dfrac{dL}{dt}+\mu_{22}\dfrac{dM}{dt}+\mu_{23}\dfrac{dN}{dt}\right)=\dfrac{dX}{dz}-\dfrac{dZ}{dx}, \\ A\left(\mu_{13}\dfrac{dL}{dt}+\mu_{23}\dfrac{dM}{dt}+\mu_{33}\dfrac{dN}{dt}\right)=\dfrac{dY}{dx}-\dfrac{dX}{dy}; \end{cases}$$

$$(5_b)\quad \begin{cases} A\left(\epsilon_{11}\dfrac{dX}{dt}+\epsilon_{12}\dfrac{dY}{dt}+\epsilon_{13}\dfrac{dZ}{dt}\right)=\dfrac{dM}{dz}-\dfrac{dN}{dy}, \\ A\left(\epsilon_{12}\dfrac{dX}{dt}+\epsilon_{22}\dfrac{dY}{dt}+\epsilon_{23}\dfrac{dZ}{dt}\right)=\dfrac{dN}{dx}-\dfrac{dL}{dz}, \\ A\left(\epsilon_{13}\dfrac{dX}{dt}+\epsilon_{23}\dfrac{dY}{dt}+\epsilon_{33}\dfrac{dZ}{dt}\right)=\dfrac{dL}{dy}-\dfrac{dM}{dx}. \end{cases}$$

The equation for the variation of the energy of a space gives the same result as in sections (3) and (4). Experience also shows that it is not necessary to regard the ϵ and μ in the equations of the present section as being constant throughout the space; they may be magnitudes varying in any way from point to point.

6. *Distribution of the Forces in Conductors*

In the bodies hitherto considered, every variation of the electric force appears as the consequence of the presence of magnetic forces. If within a finite region the magnetic forces are equal to zero, every cause for such a variation is wanting; and any existing distribution of electric force remains permanently, so long as it is left to itself and no disturbance reaches the interior from beyond the limits of the region. The electric forces do not behave thus in all bodies. In many bodies an electric force when left to itself vanishes more or less rapidly away; in such bodies magnetic forces or other causes are necessary in order to restrain an existing distribution from change. For reasons which will appear later, we call such bodies conductors. The simplest assumptions which we can make with respect to them are these: In the first place, that the loss per unit time experienced by an electric force when left to itself is proportional to the force itself; and, in the second place, that independently of this loss the magnetic forces here tend to produce the same variations as in the bodies hitherto considered. If we introduce a new constant λ, the first assumption allows us to assert that the force-component X when left to itself will vary in accordance with the equation—

$$A\epsilon\frac{dX}{dt} = -4\pi\lambda AX.$$

This first assumption is supplemented by the second as follows:—When magnetic forces are present, the variation will take place in accordance with the equation—

$$A\epsilon\frac{dX}{dt} = \frac{dM}{dz} - \frac{dN}{dy} - 4\pi\lambda AX.$$

The constant λ is called the specific conductivity of the body,

measured electrostatically. Its dimension is the reciprocal of a time. Hence the magnitude $\epsilon/4\pi\lambda$ is a time; it is the time in which the force when left to itself sinks to $1/e$ of its initial value—the so-called time of relaxation. Hr. E. Cohn first observed and drew attention to the fact [1] that it is this latter time, and not λ itself, that is a second intrinsic constant of the body under consideration; one that can be settled without ambiguity and independently of any second medium.

These considerations lead us now, conjecturally, to the following equations which are in accordance with experience:—

$$(6_a)\begin{cases} A\mu\dfrac{dL}{dt} = \dfrac{dZ}{dy} - \dfrac{dY}{dz}, \\ A\mu\dfrac{dM}{dt} = \dfrac{dX}{dz} - \dfrac{dZ}{dx}, \\ A\mu\dfrac{dN}{dt} = \dfrac{dY}{dx} - \dfrac{dX}{dy}; \end{cases} \qquad (6_b)\begin{cases} A\epsilon\dfrac{dX}{dt} = \dfrac{dM}{dz} - \dfrac{dN}{dy} - 4\pi\lambda AX, \\ A\epsilon\dfrac{dY}{dt} = \dfrac{dN}{dx} - \dfrac{dL}{dz} - 4\pi\lambda AY, \\ A\epsilon\dfrac{dZ}{dt} = \dfrac{dL}{dy} - \dfrac{dM}{dx} - 4\pi\lambda AZ. \end{cases}$$

Clearly these equations refer only to isotropic media; it is, however, unnecessary, as far as the hypotheses hitherto made are concerned, that the bodies should be homogeneous as well. But in order to represent accurately the actual distribution of the forces in a non-homogeneous body, our equations still need to be supplemented to a certain extent. For if the constitution of a body varies from point to point, the electric force when left to itself does not in general sink completely to zero, but it assumes a certain final value which is not zero. This value, whose components may be X' Y' Z', we call the electromotive force acting at the point in question. We regard this as being independent of time; in general it is greater, the greater the variation of the chemical nature of the body per unit of length. We take into account the action of the electromotive force as follows:—Instead of making the diminution of the electric force when left to itself proportional to its absolute value, we make it proportional to the difference which remains between this absolute value and the final value. In the case, then, of conductors whose structure admits of the production of electromotive forces, our equations become—

[1] With respect to this, and the manner in which the magnitude λ is here introduced, cf. E. Cohn, *Berl. Ber.* **26**, p. 405, 1889.

$$(6_c)\begin{cases} A\mu\dfrac{dL}{dt} = \dfrac{dZ}{dy} - \dfrac{dY}{dz}, \\ A\mu\dfrac{dM}{dt} = \dfrac{dX}{dz} - \dfrac{dZ}{dx}, \\ A\mu\dfrac{dN}{dt} = \dfrac{dY}{dx} - \dfrac{dX}{dy}; \end{cases} \qquad (6_d)\begin{cases} A\epsilon\dfrac{dX}{dt} = \dfrac{dM}{dz} - \dfrac{dN}{dy} - 4\pi\lambda A(X - X'), \\ A\epsilon\dfrac{dY}{dt} = \dfrac{dN}{dx} - \dfrac{dL}{dz} - 4\pi\lambda A(Y - Y'), \\ A\epsilon\dfrac{dZ}{dt} = \dfrac{dL}{dy} - \dfrac{dM}{dx} - 4\pi\lambda A(Z - Z'). \end{cases}$$

7. *Eolotropic Conductors*

If the conductor behaves differently in different directions, we can no longer assume that the diminution in each component of the force when left to itself depends only upon this same component; we must rather suppose that it is a linear function of the three components. If we further assume that when the conductivity vanishes, the equations reduce to those of an eolotropic non-conductor, we arrive at the following system:—

$$(7_a)\begin{cases} A\left(\mu_{11}\dfrac{dL}{dt} + \mu_{12}\dfrac{dM}{dt} + \mu_{13}\dfrac{dN}{dt}\right) = \dfrac{dZ}{dy} - \dfrac{dY}{dz}, \\ A\left(\mu_{12}\dfrac{dL}{dt} + \mu_{22}\dfrac{dM}{dt} + \mu_{23}\dfrac{dN}{dt}\right) = \dfrac{dX}{dz} - \dfrac{dX}{dx}, \\ A\left(\mu_{13}\dfrac{dL}{dt} + \mu_{23}\dfrac{dM}{dt} + \mu_{33}\dfrac{dN}{dt}\right) = \dfrac{dY}{dx} - \dfrac{dX}{dy}; \end{cases}$$

$$(7_b)\begin{cases} A\left(\epsilon_{11}\dfrac{dX}{dt} + \epsilon_{12}\dfrac{dY}{dt} + \epsilon_{13}\dfrac{dZ}{dt}\right) = \dfrac{dM}{dz} - \dfrac{dN}{dy} \\ \qquad - 4\pi A\{\lambda_{11}(X - X') + \lambda_{12}(Y - Y') + \lambda_{13}(Z - Z')\}, \\ A\left(\epsilon_{12}\dfrac{dX}{dt} + \epsilon_{22}\dfrac{dY}{dt} + \epsilon_{23}\dfrac{dZ}{dt}\right) = \dfrac{dN}{dx} - \dfrac{dL}{dz} \\ \qquad - 4\pi A\{\lambda_{21}(X - X') + \lambda_{22}(Y - Y') + \lambda_{23}(Z - Z')\}, \\ A\left(\epsilon_{13}\dfrac{dX}{dt} + \epsilon_{23}\dfrac{dY}{dt} + \epsilon_{33}\dfrac{dZ}{dt}\right) = \dfrac{dL}{dy} - \dfrac{dM}{dx} \\ \qquad - 4\pi A\{\lambda_{31}(X - X') + \lambda_{32}(Y - Y') + \lambda_{33}(Z - Z')\}. \end{cases}$$

It is highly probable that for all actual bodies $\lambda_{12} = \lambda_{21}$, $\lambda_{31} = \lambda_{13}$, $\lambda_{23} = \lambda_{32}$. We may regard the constants ϵ, μ, λ in the equations of this section again as varying in value from place to place.

8. *Limiting Conditions*

It is easily seen that the equations (7_a) and (7_b) include all the earlier ones as particular cases; and that even the equations for the free ether can be deduced from them by a suitable disposition of the constants. Now since these constants may be functions of the space, we are led to regard the surface of separation of two heterogeneous bodies as a transition-layer, in which the constants certainly pass with extraordinary rapidity from one value to another, but in which this still happens in such a way that even in the layer itself the above equations always hold good, and express finite relations between the finite values of the constants and the forces which also remain finite. In order to deduce the limiting conditions from this hypothesis, which is in accordance with experience, let us for the sake of simplicity suppose that the element of the surface of separation under consideration coincides with the xy-plane.

Disregarding for the moment the appearance of electromotive forces between the bodies in contact, we find, on examining the first two of the equations (7_a) and (7_b), that the magnitudes

$$\frac{dX}{dz}, \quad \frac{dY}{dz}, \quad \frac{dM}{dz}, \quad \frac{dL}{dz}$$

must, in consequence of our hypothesis, remain finite in the transition-layer as well. Thus, if the index 1 refers to the one side of the limiting layer, and the index 2 to the other side, we must have

$$(8_a) \quad \begin{matrix} Y_2 - Y_1 = 0, \\ X_2 - X_1 = 0. \end{matrix} \qquad (8_b) \quad \begin{matrix} M_2 - M_1 = 0, \\ L_2 - L_1 = 0. \end{matrix}$$

The components of the force which are tangential to the limiting surface therefore continue through it without discontinuity. Applying this to the third of the equations (7_a) and (7_b), we further find that the expressions

$$\mu_{13}\frac{dL}{dt} + \mu_{23}\frac{dM}{dt} + \mu_{33}\frac{dN}{dt} \quad \text{and}$$

$$\epsilon_{13}\frac{dX}{dt} + \epsilon_{23}\frac{dY}{dt} + \epsilon_{33}\frac{dZ}{dt} + 4\pi(\lambda_{31}\dot{X} + \lambda_{32}Y + \lambda_{33}Z)$$

must have the same value on both sides of the limiting layer. This statement, which expresses the reciprocal dependence of the normal components of the forces on both sides of the limiting surface, assumes in the case of isotropic bodies the simple form

$$(8_c)\quad \mu_1 \frac{dN_1}{dt} - \mu_2 \frac{dN_2}{dt} = 0,$$

$$(8_d)\quad \epsilon_1 \frac{dZ_1}{dt} - \epsilon_2 \frac{dZ_2}{dt} = -4\pi(\lambda_1 Z_1 - \lambda_2 Z_2).$$

In the next place, if we do not exclude the appearance of electromotive forces in the limiting layer, we have to observe that, in accordance with experience, the component of these forces which is normal to the limiting surface, *i.e.* Z', becomes infinite in the transition-layer itself; and yet in such a way that the integral $\int Z' dz$ taken through the limiting surface retains a finite value; this value we can obtain from experiments, although these leave us in the dark as to the course of Z' itself. We now satisfy the hypothesis of the present section by assuming that, with L, M, N, X, Y, the magnitude $Z - Z'$ remains finite in the transition-layer. Z becomes infinite there; nevertheless, we can allow dZ/dt to remain finite. Furthermore, we put

$$(8_e)\quad \int Z dz = \int Z' dz = \phi_{1,2},$$

Let us now integrate the first two of the equations (7_a) and (7_b) after multiplying by dz through the transition-layer. Since, on account of the shortness of the path, the integral of every finite magnitude vanishes, we obtain the conditions—

$$(8_f)\quad \begin{cases} Y_2 - Y_1 = \dfrac{d\phi_{1,2}}{dy}, \\ X_2 - X_1 = \dfrac{d\phi_{1,2}}{dx}; \end{cases} \qquad (8_g)\quad \begin{cases} M_2 - M_1 = 0, \\ N_2 - N_1 = 0. \end{cases}$$

Applying these to the third of the equations (7_a) and (7_b), we obtain as the conditions for the normal-forces, that on both sides of the limiting surface, the values of the expressions

$$\mu_{13}\frac{dL}{dt}+\mu_{23}\frac{dM}{dt}+\mu_{33}\frac{dN}{dt},$$

$$\epsilon_{13}\frac{dX}{dt}+\epsilon_{23}\frac{dY}{dt}+\epsilon_{33}\frac{dZ}{dt}+4\pi\{\lambda_{31}(X-X')+\lambda_{32}(Y-Y')+\lambda_{33}(Z-Z')\}$$

must be equal. If the bodies on both sides of the limiting surface are homogeneous, then the presence of the electromotive forces has no effect upon the conditions by which the forces existing on the two sides are connected.

Our limiting conditions are nothing else than the general equations (7_a) and (7_b), transformed to suit special circumstances. We may, therefore, imagine every statement and every operation relating to these general equations within a definite region to be at once extended to the limits of heterogeneous bodies within the region; provided always that this procedure does not land us in mathematical impossibilities, and therefore that our statements and operations, either directly or after suitable transformation, do not cease to be finite and definite. We shall often avail ourselves of the convenience which arises from this. And if, in general, we dispense with proving that all the expressions which arise are finite and definite, it must not be supposed that this is because we regard such proof as superfluous, but only because the proof has long since been furnished, or can be supplied according to known examples, in all the cases which have to be considered.

Each one of the previous sections means an increase in the number of facts embraced by the theory. The following sections, on the other hand, deal only with names and notations. As their introduction does not increase the number of facts embraced, they are merely accessory to the theory; their value consists partly in making possible a more concise mode of expression, and partly also in simply bringing our theory into its proper relation towards the older views as to electrical theory.

9. *Electric and Magnetic Polarisation*

So far as our equations relate to isotropic media, each separate one gives the value which a single one of the physical magnitudes under consideration will have at the next moment, expressed as a definite function of the conditions existing at the present moment. This form of the equations is very perfect from a mathematical point of view, for it enables us to ascertain from the outset that the equations determine without ambiguity the course of any process whatever that may be arbitrarily excited. It also appears very perfect from a much more philosophic standpoint; for it enables us to recognise on the left-hand side of the equation the future state—the consequence—while, at the same time, on the right-hand side of the equation, it exhibits the present state—the cause thereof. But those of our equations which relate to eolotropic bodies have not this perfect form; for, on the left-hand side, they do not contain the variations of a single physical magnitude, but functions of such variations. Since these functions are linear, the equations can certainly be thrown into the desired form by solving them for the separate variations. Another means to the same end—which, at the same time, simplifies the equations—is by introducing the magnitudes which we call polarisations. We put

$$(9_c)\begin{cases} \mathfrak{L} = \mu_{11}\mathrm{L} + \mu_{12}\mathrm{M} + \mu_{13}\mathrm{N}, \\ \mathfrak{M} = \mu_{12}\mathrm{L} + \mu_{22}\mathrm{M} + \mu_{23}\mathrm{N}, \\ \mathfrak{N} = \mu_{13}\mathrm{L} + \mu_{23}\mathrm{M} + \mu_{33}\mathrm{N}; \end{cases} \qquad (9_d)\begin{cases} \mathfrak{X} = \epsilon_{11}\mathrm{X} + \epsilon_{12}\mathrm{Y} + \epsilon_{13}\mathrm{Z}, \\ \mathfrak{Y} = \epsilon_{12}\mathrm{X} + \epsilon_{22}\mathrm{Y} + \epsilon_{23}\mathrm{Z}, \\ \mathfrak{Z} = \epsilon_{13}\mathrm{X} + \epsilon_{23}\mathrm{Y} + \epsilon_{33}\mathrm{Z}; \end{cases}$$

and call the resultant of $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ the magnetic, and the resultant of $\mathfrak{X}$, $\mathfrak{Y}$, $\mathfrak{Z}$ the electric polarisation. For isotropic bodies the polarisations and the forces have the same direction, and the ratio of the former to the latter is the specific inductive capacity or the magnetic permeability. In the case of the ether polarisations and forces coincide. If we introduce the polarisations on the left-hand side of our equations, then each equation gives us the variation of a single polarisation-component as the result of the forces instantaneously present. Since the forces are linear functions of the polarisation, there is no difficulty in introducing the polarisations on the right-hand side of the equations as well. We should thus have

replaced the particular directed magnitude—the force—which we first used to determine the electromagnetic state, by another directed magnitude—the polarisation—which would serve our purpose as well, but not any better. The introduction of the polarisations and the forces side by side considerably simplifies the equations; and this is our first indication that, in order to represent completely the conditions in ponderable bodies, it is necessary to specify at least two directed magnitudes for the electrical condition and two for the magnetic condition.

In order to simplify our equations further, let us put

$$(9_e)\begin{cases} u = \lambda_{11}(X - X') + \lambda_{12}(Y - Y') + \lambda_{13}(Z - Z'), \\ v = \lambda_{21}(X - X') + \lambda_{22}(Y - Y') + \lambda_{23}(Z - Z'), \\ w = \lambda_{31}(X - X') + \lambda_{32}(Y - Y') + \lambda_{33}(Z - Z'). \end{cases}$$

For reasons which will appear in the next section, we call u, v, w the components (measured electrostatically) of the electric current-density.

Our most general equations now take the form

$$(9_a)\begin{cases} A\frac{d\mathfrak{X}}{dt} = \frac{dZ}{dy} - \frac{dY}{dz}, \\ A\frac{d\mathfrak{M}}{dt} = \frac{dX}{dz} - \frac{dZ}{dx}, \\ A\frac{d\mathfrak{N}}{dt} = \frac{dY}{dx} - \frac{dX}{dy}; \end{cases} \qquad (9_b)\begin{cases} A\frac{d\mathfrak{X}}{dt} = \frac{dM}{dz} - \frac{dN}{dy} - 4\pi A u, \\ A\frac{d\mathfrak{Y}}{dt} = \frac{dN}{dx} - \frac{dL}{dz} - 4\pi A v, \\ A\frac{d\mathfrak{Z}}{dt} = \frac{dL}{dy} - \frac{dM}{dx} - 4\pi A w, \end{cases}$$

and, on introducing the polarisations, the electromagnetic energy per unit volume of any body whatever takes the form

$$\frac{1}{8\pi}(\mathfrak{X}X + \mathfrak{Y}Y + \mathfrak{Z}Z) + \frac{1}{8\pi}(\mathfrak{L}L + \mathfrak{M}M + \mathfrak{N}N).$$

In these expressions there no longer appear any quantities which refer to any particular body. The statement that these equations must be satisfied at all points of infinite space, embraces all problems of electromagnetism; and the infinite multiplicity of these problems only arises through the fact that the constants ϵ, μ, λ, X', Y', Z' of the linear relations (9_c), (9_d), (9_e) may be functions of the space in a multiplicity of ways, varying partly continuously, and partly discontinuously, from point to point.

10. *Electricity and Magnetism*

Let there be a system of ponderable bodies in which electromagnetic processes take place, and which are separated by empty space from other systems. Let us differentiate the three equations (9_b) with respect to x, y, z respectively, and add; we thus obtain for all points of the system the equation

$$\frac{d}{dt}\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)=-4\pi\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right).$$

Let us multiply this equation by the volume-element $d\tau$, and integrate over the volume up to any surface, completely enclosing the ponderable system. Let $d\omega$ be the element of this surface, and let the normal to $d\omega$ make with the axes the angles n,x, n,y, n,z. Since u, v, w are zero at the surface, we get

$$\frac{d}{dt}\int\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)d\tau=\frac{d}{dt}\int(\mathfrak{X}\cos n,x+\mathfrak{Y}\cos n,y+\mathfrak{Z}\cos n,z)d\omega$$

$$=-4\pi\int\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)d\tau$$

$$=-4\pi\int(u\cos n,x+v\cos n,y+w\cos n,z)d\omega=0.$$

Hence, if e denotes a quantity which is independent of time—

$$(10_a)\quad\begin{cases}\displaystyle\int\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)d\tau\\ =\int(\mathfrak{X}\cos n,x+\mathfrak{Y}\cos n,y+\mathfrak{Z}\cos n,z)d\omega=4\pi e.\end{cases}$$

The quantity e is obviously a function of the electrical state of the system—a function of such a kind that it cannot be increased or diminished by any internal or external processes of a purely electromagnetic nature. This indestructibility of the quantity e—which also holds good for other than purely electromagnetic processes, so long as these are restricted to the interior of the system—has prompted the idea that e represents the amount of some substance contained in the system. In accordance with this idea we call e the amount of electricity contained in the ponderable system. But we must allow e to

be positive or negative, whereas the amount of a substance is necessarily positive. For this reason the hypothesis has been supplemented by assuming the existence of two electricities of opposite properties, and making e mean the difference between the two; or else a way out of the difficulty has been sought in assuming that e represents only the deviation of the amount of electricity actually contained from the normal amount. But if e represents the quantity of a substance in one of these forms or some other form, then each volume-element $d\tau$ must furnish its definite share towards the total amount of e. Only hypothetically can we distribute the volume-integral, which supplies e, among the separate volume-elements. A possible distribution—the first which suggests itself for the moment—is that which assigns to the volume-element $d\tau$ the quantity of electricity—

$$\frac{1}{4\pi}\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)d\tau.$$

We shall call the quantity of electricity thus determined the true electricity of the volume-element; in the interior of a body, in accordance with this, we shall call the expression

$$\frac{1}{4\pi}\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)$$

the true volume-density, and at the surface of separation of dissimilar bodies the expression

$$\frac{1}{4\pi}\{(\mathfrak{X}_2-\mathfrak{X}_1)\cos n,x+(\mathfrak{Y}_2-\mathfrak{Y}_1)\cos n,y+(\mathfrak{Z}_2-\mathfrak{Z}_1)\cos n,z\}$$

the true surface-density of the electricity.

Another possible distribution of e among the volume-elements which suggests itself is that which we get through observing that in empty space polarisations and forces are identical, and that we can therefore write instead of (10_a)—

$$(10_b)\quad\begin{cases}4\pi e=\int(\mathrm{X}\cos n,x+\mathrm{Y}\cos n,y+\mathrm{Z}\cos n,z)d\omega\\ \quad=\int\left(\dfrac{d\mathrm{X}}{dx}+\dfrac{d\mathrm{Y}}{dy}+\dfrac{d\mathrm{Z}}{dz}\right)d\tau,\end{cases}$$

and furthermore, that we can regard the expression

$$\frac{1}{4\pi}\left(\frac{dX}{dx}+\frac{dY}{dy}+\frac{dZ}{dz}\right)d\tau$$

as representing the share which the volume-element $d\tau$ contributes to e. Accordingly, we call the quantity of electricity so determined the free electricity of the volume-element, and

$$\frac{1}{4\pi}\left(\frac{dX}{dx}+\frac{dY}{dy}+\frac{dZ}{dz}\right)$$

the free volume-density, and at surfaces of separation—

$$\frac{1}{4\pi}\{(X_2-X_1)\cos n,x+(Y_2-Y_1)\cos n,y+(Z_2-Z_1)\cos n,z\}$$

the free surface-density of the electricity. The difference between the true and the free electricity we call the bound electricity. Our nomenclature follows the familiar nomenclature which takes its origin from the view hitherto held as to the existence of electrical action at-a-distance.[1] According to this view, a part of the extraneous or "true" amount of electricity introduced into a non-conductor remains "bound" by electrical displacement[2] in the molecules of the surrounding medium; whereas the rest remains "free" to exert its distance-action outwards. And yet in many respects our nomenclature differs from the usual one. But since the latter is sometimes ambiguous and not always consistent, it was not possible for me to find a system of notation which would in all cases harmonise with the common use of terms. The common phraseology is also ambiguous in that it uses the word electricity without further discrimination to denote sometimes the true, sometimes the free electricity; and this even when important statements are being made.

In accordance with what has been stated above, we denote the integral

$$\int(\mathfrak{X}\cos n,x+\mathfrak{Y}\cos n,y+\mathfrak{Z}\cos n,z)d\omega,$$

extended over any closed surface and divided by 4π, as the

[1] [See Note 30 at end of book.]

[2] This is not identical with our polarisations. [See the theoretical part of the Introduction.]

true electricity contained within this surface. The same integral extended over an unclosed surface we shall call the number of electric lines of force traversing this surface in the direction of the positive normal. By this notation we follow Faraday's conception, according to which the lines of force are lines which in isotropic homogeneous bodies run everywhere in the direction of the prevailing force, and the number of which is proportional to the magnitude of the force. It is true that by our notation we have rendered this conception more complete or precise in this respect,—that in all bodies we make the lines of force run everywhere in the direction of the polarisation, not of the force, and that their density is in all cases proportional to the magnitude of the polarisation, not of the force. It follows from our definitions that the quantity of true electricity contained in any space, multiplied by 4π, is equal to the excess of the number of lines of force which enter the surface over the number which leave it. Every line of force which has an end must accordingly end in true electricity; and we may define the true electricity as the free ends of the lines of force. If a given space in the neighbourhood of the surface over which our integral extends is free from true electricity, then the value of the integral is independent of the particular position of the surface within this space; it only depends upon the position of the boundary of the surface. In this case, then, we denote the value of the integral as being the number of lines of force crossing the boundary line—any ambiguity remaining in this expression being supposed removed by special restrictions.

We shall next calculate the variation of the true electricity e_w in a part of our system bounded in any way. Let $d\omega$ again be an element of the bounding surface of this part. We get

$$(10_c)\quad \frac{de_w}{dt} = -\int\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)d\tau = -\int(u\cos n,x + v\cos n,y + w\cos n,z)d\omega.$$

Now if our bounding surface runs entirely in bodies for which λ is equal to zero, then u, v, w still vanish at the surface, and hence the amount of true electricity contained in the space bounded by it remains constant. Accordingly, true electricity cannot by any purely electromagnetic process escape from a space which is wholly bounded by bodies for which λ is

equal to zero. For this reason we call, and have called, such bodies non-conductors. But if the bounding surface passes wholly or partially through bodies for which λ is not zero, it becomes possible for the amount of electricity within the space so bounded to vary through purely electrical disturbances; for this reason we call bodies of this latter kind conductors. This division of bodies into conductors and non-conductors has reference therefore to the true electricity; with reference to the free electricity all bodies may be regarded as conductors (cf. displacement-currents). The amount of a substance within a given space can only vary by its passing inwards or outwards through the surface; and it is clear that a definite amount of the substance must pass through each element of the surface. Consistently with the fact that the amount of electricity given by our integral passes per unit time through every closed surface, we may assume that the amount

$$u \cos n,x + v \cos n,y + w \cos n,z$$

passes through unit surface of every surface-element. In accordance with this assumption we call, and have called, u, v, w the components of the electric current-density, and the integral

$$\int (u \cos n,x + v \cos n,y + w \cos n,z) d\omega,$$

taken over an unclosed surface, the electric current flowing through this surface. We must, however, lay stress upon this—that even if we admit the materiality of electricity, the above special determination of its flow in conductors embraces a further hypothesis. Upon the system of disturbance found there can be superposed an arbitrary current-system, closed at every moment, without thereby altering the increase or decrease of electricity at any point.

If a portion of our system has attained its present condition, starting from the unelectrified condition, by purely electromagnetic processes, or if by purely electromagnetic changes it can return to the unelectrified state, then in all non-conductors of this portion the true electricity is equal to zero. For such portions we have, then, in addition to the general equations, the following as limitations of the permissible initial conditions which are not inconsistent with the general equations, viz.:—

$$\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}=0$$

for the interior of non-conductors; and

$$(\mathfrak{X}_2-\mathfrak{X}_1)\cos n,x+(\mathfrak{Y}_2-\mathfrak{Y}_1)\cos n,y+(\mathfrak{Z}_2-\mathfrak{Z}_1)\cos n,z=0$$

for the boundary between two heterogeneous non-conductors.

The magnetic phenomena can be considered in a manner exactly analogous to the electric phenomena. Let us proceed to examine these, with the assistance of the equations (9_a). We shall call

$$\frac{1}{4\pi}\left(\frac{d\mathfrak{L}}{dx}+\frac{d\mathfrak{M}}{dy}+\frac{d\mathfrak{N}}{dz}\right)$$

the true volume-density for the interior of a body; the expression—

$$\frac{1}{4\pi}\{(\mathfrak{L}_2-\mathfrak{L}_1)\cos n,x+(\mathfrak{M}_2-\mathfrak{M}_1)\cos n,y+(\mathfrak{N}_2-\mathfrak{N}_1)\cos n,z\}$$

the true surface-density of magnetism at the surface of separation of two bodies; and the integral of these magnitudes extended over a definite portion of space, the true magnetism contained in this portion. The integral

$$\int(\mathfrak{L}\cos n,x+\mathfrak{M}\cos n,y+\mathfrak{N}\cos n,z)d\omega,$$

taken over an unclosed surface, we shall call the number of magnetic lines of force penetrating this surface, or the boundary of this surface. Further, we shall call

$$\frac{1}{4\pi}\left(\frac{d\mathrm{L}}{dx}+\frac{d\mathrm{M}}{dy}+\frac{d\mathrm{N}}{dz}\right)$$

the free volume-density for the interior of a body; and

$$\frac{1}{4\pi}\{(\mathrm{L}_2-\mathrm{L}_1)\cos n,x+(\mathrm{M}_2-\mathrm{M}_1)\cos n,y+(\mathrm{N}_2-\mathrm{N}_1)\cos n,z\}$$

the free surface-density of the magnetism at the surface of separation of two bodies. The distinction between conductors and non-conductors here disappears; for the equations (9_a) contain no terms corresponding to the u, v, w of equations (9_b). With respect to true magnetism all bodies are non-conductors;

with respect to free magnetism all bodies may be regarded as conductors.

Let us suppose that a system or a portion of it emerges from the non-magnetic state through purely electromagnetic processes, or can by such processes return to this state. For such a system, or that portion of it, the following equations obtain, viz.—

$$\frac{d\mathfrak{L}}{dx}+\frac{d\mathfrak{M}}{dy}+\frac{d\mathfrak{N}}{dz}=0$$

for the interior of the bodies; and

$$(\mathfrak{L}_2-\mathfrak{L}_1)\cos n,x+(\mathfrak{M}_2-\mathfrak{M}_1)\cos n,y+(\mathfrak{N}_2-\mathfrak{N}_1)\cos n,z=0$$

for the surface of separation of heterogeneous bodies. These are supplementary to the general equations as consistent stipulations respecting the possible initial conditions.

11. *Conservation of Energy*

Let S denote the electromagnetic energy of a volume τ, which is bounded by the surface ω. We can calculate the variation of S by multiplying all the equations (9_a) and (9_b) by $(1/4\pi A)d\tau$, then multiplying them separately in order by L, M, N; X, Y, Z, adding all together, and integrating over the volume τ. We obtain

$$(11_a)\begin{cases}\dfrac{dS}{dt}=\dfrac{1}{4\pi A}\int\{(NY-MZ)\cos n,x+(LZ-NX)\cos n,y\\ \quad+(MX-LY)\cos n,z\}d\omega-\int(uX+vY+wZ)d\tau.\end{cases}$$

If we extend the space τ over a complete electromagnetic system, *i.e.* as far as a surface at which the forces vanish, then our equation becomes

$$\frac{dS}{dt}=-\int(uX+vY+wZ)d\tau.$$

The conservation of energy accordingly requires that in every system which is not subjected to external actions, an amount of energy corresponding to the integral on the right-hand side should make its appearance per unit time in other

than electromagnetic form. This requirement is satisfied by experience, which further teaches us that each separate volume-element $d\tau$ furnishes towards the total amount of the transformed energy the quantity

$$(uX + vY + wZ)d\tau,$$

and shows us in what form this energy makes its appearance. Or rather, to speak accurately, experience does not show that this is true in all cases, but provisionally in the following special cases only. According to both theory and experience, the amount of energy which appears per unit time and per unit volume in the interior of a homogeneous isotropic conductor takes the form

$$\lambda(X^2 + Y^2 + Z^2) = \frac{1}{\lambda}(u^2 + v^2 + w^2).$$

It is always positive and represents a development of heat —the Joule effect. At the boundary between two homogeneous isotropic bodies, the amount of energy per unit volume that appears in the transition-layer takes the form

$$uX' + vY' + wZ';$$

hence, by integration over the whole thickness of the transition-layer, it follows that the quantity of energy which appears per unit of surface at the boundary amounts to

$$(u \cos n,x + v \cos n,y + w \cos n,z) \cdot \phi_{1,2},$$

which expression is also confirmed by experience. This expression may be either positive or negative; it may correspond either to an appearance or a disappearance of foreign forms of energy. Either the transformed foreign energy is heat in this case as well—the Peltier effect; in which case we denote the effective electromotive forces as thermoelectric. Or else chemical energy as well as heat is transformed; in which case we denote the forces as electrochemical. Let us now consider any limited portion of our system and calculate for it the increase of its total energy, *i.e.* of the quantity

$$\frac{dS}{dt} + \int(uX + vY + wZ)d\tau.$$

In accordance with what has been stated, we find that this

increase is equal to an integral taken over the surface of the space. The variation of the stock of energy in this (and therefore in any) space will therefore be correctly calculated if we assume that the energy enters after the manner of a substance through the surface, and in such quantity that through every such surface the amount

$$\frac{1}{4\pi A}\{(NY - MZ)\cos n,x + (LZ - NX)\cos n,y + (MX - LY)\cos n,z\}$$

enters per unit of surface. A geometrical discussion of this expression shows that our assumption is identical with the statement that the energy moves everywhere in a direction perpendicular to the directions of the magnetic and electric forces, and in such amount that in this direction a quantity equal to the product of the two forces, the sine of the enclosed angle, and the factor $1/4\pi A$, passes through unit surface per unit time. This is Dr. Poynting's highly remarkable theory on the transfer of energy in the electromagnetic field.[1] In examining its physical meaning we must not forget that our analysis of the surface-integral into its elements was hypothetical, and that the result thereof is not always probable. If a magnet remains permanently at rest in presence of an electrified body, then in accordance with this result the energy of the neighbourhood must find itself in a state of continuous motion, going on, of course, in closed paths. In the present state of our knowledge respecting energy there appears to me to be much doubt as to what significance can be attached to its localisation and the following of it from point to point. Considerations of this kind have not yet been successfully applied to the simplest cases of transference of energy in ordinary mechanics; and hence it is still an open question whether, and to what extent, the conception of energy admits of being treated in this manner.[2]

12. *Ponderomotive Forces*

The mechanical forces, which we perceive between ponderable bodies in the electromagnetically stressed field, we regard

[1] J. H. Poynting, *Phil. Trans.* **2**, p. 343, 1884.
[2] [See Note 31 at end of book.]

as the resultants of mechanical pressures which are excited by the existence of electromagnetic stresses in the ether and in other bodies. According to this view the mechanical forces which act upon a ponderable body are completely determined by the electromagnetic state of its immediate neighbourhood; and it need not be considered what causes at a distance may have led up to this state. We further assume that the presupposed pressures are of such a kind that they cannot give rise to any resultants which would tend to set the interior of the ether itself in motion. Without this hypothesis our system would necessarily be incorrect, or at least incomplete; for without it we could not in general speak at all of electromagnetic forces in the ether at rest. It necessarily follows from this hypothesis that the forces under observation, acting upon ponderable bodies, must satisfy the principle of the equality of action and reaction.

The question now is—Whether pressures can be specified answering these requirements, and capable of producing the resultants which are actually observed? Maxwell and, in a more general form, von Helmholtz have described forms of pressures which satisfy all the requirements of statical and stationary states. But these pressures, if assumed to obtain for the general variable state, would set the ether itself in motion. We therefore assume that the complete forms have not been discovered, and, avoiding any definite statements as to the magnitude of the pressures, we shall rather deduce the ponderomotive forces with the aid of the hypotheses already stated, of the principle of the conservation of energy, and of the following fact derived from experience:—If the ponderable bodies of an electrically or magnetically excited system, which always remains indefinitely near to the statical condition, are displaced with reference to one another, and if at the same time the amount of true electricity and of true magnetism in each element of the bodies remains invariable and behaves as if attached to the element, then the mechanical work consumed in the displacement of the bodies finds its only compensation in the increase of the electromagnetic energy of the system, and is therefore equal to this latter.[1]

It still remains an open question whether forms of pressure can be specified which satisfy, generally and precisely, the

[1] [See Note 32 at end of book.]

requirements which we have laid down. If this is not the case, our body of hypotheses contains an intrinsic contradiction which must be removed by correcting one or more of these hypotheses. But at all events the necessary amendments are of such a kind that their effect would not make itself felt in any of the phenomena hitherto observed. And it must be pointed out that if there is here something lacking in our theory, it is not a defect in the foundations of the theory, but in parts of the superstructure. For, from our point of view, the mechanical forces excited are secondary consequences of the electromagnetic forces. We could discuss the theory of the latter without even mentioning the former; as indeed we have excluded from the discussion all other phenomena of minor importance which result from the electromagnetic state.

B. Deduction of the Phenomena from the Fundamental Equations

We divide the phenomena represented by our equations into statical, stationary, and dynamical. In order that a phenomenon may rank as statical or stationary, it is necessary that it should not determine any variations of the electric and magnetic forces with time, *i.e.* that the left-hand sides of the equations (9_a) and (9_b) should vanish. Furthermore, in order that a phenomenon may rank as statical, it is also necessary that it should not be accompanied by changes in time at all, and hence, more especially, that it should not determine any permanent change of energy into other forms. The sufficient and necessary condition for this is that the quantities u, v, w in equations (9_a) and (9_b) should also vanish.

Statical Phenomena

If in the equations (9_a) and (9_b) the left-hand sides and also the quantities u, v, w vanish, the system splits up into two mutually independent systems, of which one contains only the electric forces and the other only the magnetic forces. We thus get two groups of problems, of which one is called electrostatics, and the other might be called magnetostatics.

13. *Electrostatics*

In this section we shall disregard the occurrence of electromotive forces; for if these admit of the existence of the statical state at all, their action is too weak to come into consideration in the problems which are of interest. In conductors, accordingly, in which the quantities λ do not vanish, the forces X, Y, Z must vanish. In non-conductors the equations (9_a) take the form

$$(13_a)\quad \frac{dZ}{dy}-\frac{dY}{dz}=\frac{dX}{dz}-\frac{dZ}{dx}=\frac{dY}{dx}-\frac{dX}{dy}=0.$$

Hence the forces possess a potential ϕ, and can be put equal to the negative differential coefficient of this potential. Since the forces are everywhere finite, ϕ is everywhere continuous; it can therefore continue right through the conductors, and is then to be regarded as constant within these. At a surface of separation the differential coefficients of ϕ tangential to the separating surface continue through it without discontinuity. Again, if e_f denote the volume-density of the free electricity, according to section (10) the potential ϕ satisfies everywhere in space the equation $\Delta\phi=-4\pi e_f$. In free ether this assumes the form $\Delta\phi=0$; and after suitable transformation for the surface of separation between heterogeneous bodies it gives the condition

$$\left(\frac{d\phi}{dn}\right)_2-\left(\frac{d\phi}{dn}\right)_1=-4\pi e'_f,$$

where e'_f denotes the surface-density of the free electricity. From all these conditions it follows that the value of ϕ, within an arbitrary constant, is definite and equal to $\int(e_f/r)d\tau$, the integral being extended over the whole space with due regard to the surfaces of separation. Thus when the potential and the forces are distributed in the same way in different non-conductors, the free electricities are the same. But the corresponding quantities of true electricity are different, and for the interior of two homogeneous non-conductors they are in the ratio of the specific inductive capacities. Restricting ourselves for the moment to isotropic bodies, the condition

that the density of the true electricity in the interior of the non-conductors should have given values e_w, is expressed by the equation

$$\frac{d}{dx}\left(\epsilon\frac{d\phi}{dx}\right)+\frac{d}{dy}\left(\epsilon\frac{d\phi}{dy}\right)+\frac{d}{dz}\left(\epsilon\frac{d\phi}{dz}\right)=-4\pi e_w,$$

which at the boundary of two isotropic bodies assumes the form

$$\epsilon_2\left(\frac{d\phi}{dn}\right)_2-\epsilon_1\left(\frac{d\phi}{dn}\right)_1=-4\pi e'_w,$$

where e'_w denotes the surface-density of the true electricity.

Let us now direct our attention to the stock of energy in an electrostatic system. We obtain this successively in the forms

$$\frac{1}{8\pi}\int(\mathfrak{X}X+\mathfrak{Y}Y+\mathfrak{Z}Z)d\tau=-\frac{1}{8\pi}\int\left(\mathfrak{X}\frac{d\phi}{dx}+\mathfrak{Y}\frac{d\phi}{dy}+\mathfrak{Z}\frac{d\phi}{dz}\right)d\tau$$

$$=\frac{1}{8\pi}\int\phi\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)d\tau=\tfrac{1}{2}\int\phi e_w d\tau=\tfrac{1}{2}\int\int\frac{e_w e_f}{r}d\tau d\tau.$$

The integrations are here supposed to extend over all space in which electrical stresses exist, and therefore up to the boundaries where the stresses vanish, and the suitable transformation of the integrals at the bounding surfaces is implicitly assumed. When any motion of the ponderable bodies takes place, and the amounts of true electricity attached to the elements of these bodies remain constant, then, according to section (12), the increase in the value of any one of these expressions is equal to the work done by the mechanical forces in this motion. Hence, if our system consists of two quantities of electricity E_1 and E_2, separated by the ether and at a distance R apart which is very great compared with their own dimensions, and if their distance apart increases by an amount of dR, the electric energy of the space decreases by an amount

$$\tfrac{1}{2}(E_1E_2+E_2E_1)\frac{d\mathrm{R}}{\mathrm{R}^2}.$$

Thus the expression E_1E_2/R^2 represents the mechanical force with which the two electricities tend to move apart.

Coulomb's law, which in the older theories forms the starting-point of every discussion, here makes its appearance as a remote final result.

With regard to the general determination of the ponderomotive forces, we must here content ourselves with the following remarks:—The last two expressions obtained for the energy are just those whose variations represent the work done by the motion of bodies in ordinary electrostatics. Hence it follows that from the variations of these expressions we can calculate the values of those same forces which are the starting-point of ordinary electrostatics and are tested by experiment. In particular, it may be shown that an element of a body which contains a quantity e of true electricity is acted upon by the mechanical force-components eX, eY, eZ. We thus return to the same statements by means of which we first introduced the electric forces.

14. *Magnetostatics*

The equations which connect the components of statical magnetic forces are the same as those which obtain between the components of statical electric forces. Hence all the statements in the preceding section may, with the necessary changes of notation, be repeated here. And if, nevertheless, the magnetic problems of interest are still distinct mathematically from the electrostatic problems, this arises from the following causes:—

(1) The class of bodies known as conductors is here wanting.

(2) In no bodies, excepting those which exhibit permanent or remanent magnetism, does true magnetism appear. Hence in the interior of such bodies, provided they are isotropic, the magnetic potential ψ must necessarily and always satisfy the equation

$$\frac{d}{dx}\left(\mu\frac{d\psi}{dx}\right)+\frac{d}{dy}\left(\mu\frac{d\psi}{dy}\right)+\frac{d}{dz}\left(\mu\frac{d\psi}{dz}\right)=0,$$

which at the boundary between two such bodies becomes

$$\mu_2\left(\frac{d\psi}{dn}\right)_2-\mu_1\left(\frac{d\psi}{dn}\right)_1=0.$$

The equations which apply to the interior and the boundaries of crystalline bodies are somewhat more complicated, but can easily be given; these equations have to be considered in discussing the phenomena of the so-called magne-crystallic force.

(3) The specific inductive capacity of all known bodies is greater than unity; on the other hand, the magnetic permeability of many bodies is less than unity. We call such bodies diamagnetic, and all others paramagnetic. The free magnetic density at the surface of an isotropic body bounded by empty space is equal to $(1 - \mu)$ times the force in the interior of the body normal to the surface. The sign of the surface-magnetism (*Belegung*) of a diamagnetic body is therefore opposite to that of a paramagnetic body when the sense of the force is the same.

The study of statical magnetism further acquires a peculiar aspect, owing to the fact that iron and steel, which are the most important substances in connection with magnetic phenomena, do not fit in at all well with the theoretical treatment. These substances exhibit permanent and remanent magnetism; hence the polarisation of the ponderable material is here partly independent of the prevailing force, and therefore the magnetic state cannot be completely defined by a single directed magnitude. Again, the relations between the force and the disturbances produced by it are not linear; so that, for a double reason, our theory does not include these bodies entirely within its scope. In order to avoid excluding them entirely from consideration, we replace them by the two ideal substances which approximate most nearly to them—perfectly soft iron and perfectly hard steel. We define the first as a substance which obeys our equations, and for which the value of μ is very large. We attain a nearer approximation by giving μ different values according to the problem under consideration. We define perfectly hard steel as a substance which obeys our equations, whose magnetic permeability is unity, in whose interior true magnetism can exist distributed in any way, provided always that the total quantity of true magnetism existing in any such piece of steel does not differ from zero.

Stationary States

The same conditions hold good for the state of stationary disturbances in non-conductors as for the statical condition; in

conductors, which for the sake of simplicity we shall assume in this section to be isotropic, the equations (9_a), (9_b), (9_c), which here come under consideration, take the form

$$(15_a)\begin{cases}\dfrac{dZ}{dy}-\dfrac{dY}{dz}=0,\\ \dfrac{dX}{dz}-\dfrac{dZ}{dx}=0,\\ \dfrac{dY}{dx}-\dfrac{dX}{dy}=0;\end{cases}\qquad (15_b)\begin{cases}\dfrac{dM}{dz}-\dfrac{dN}{dy}=4\pi A u,\\ \dfrac{dN}{dx}-\dfrac{dL}{dz}=4\pi A v,\\ \dfrac{dL}{dy}-\dfrac{dM}{dx}=4\pi A w.\end{cases}$$

$$(15_c)\quad u=\lambda(X-X'),\quad v=\lambda(Y-Y'),\quad w=\lambda(Z-Z').$$

Differentiating equations (15_b) with respect to x, y, z respectively, and adding, we get

$$(15_d)\quad \frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0,$$

which equation, at surfaces where the currents vary abruptly, takes the form

$$(15_e)\quad (u_2-u_1)\cos n,x+(v_2-v_1)\cos n,y+(w_2-w_1)\cos n,z=0.$$

Combining equations (15_d) and (15_e) with equations (15_a) and (15_c), we obtain a system which contains only the electric forces. This can be treated without regard to the magnetic forces, and gives us the theory of current-distribution. If the components u, v, w of the current are found, the treatment of the equations (15_b) further gives us the magnetic forces exerted by these currents.

15. *Distribution of Steady Currents*

It appears from equations (15_a) that, in the interior of the conductor through which a current is flowing, the forces can also be represented as the negative differential coefficients of a function ϕ, the potential, which is determined by the following condition, which must obtain everywhere :—

$$(15_f)\quad \frac{d}{dx}\left(\lambda\frac{d\phi}{dx}\right)+\frac{d}{dy}\left(\lambda\frac{d\phi}{dy}\right)+\frac{d}{dz}\left(\lambda\frac{d\phi}{dz}\right)=-\frac{d}{dx}(\lambda X')-\frac{d}{dy}(\lambda Y')-\frac{d}{dz}(\lambda Z').$$

At the surface separating two heterogeneous conductors this equation takes the form

$$(15_g)\begin{cases} \lambda_2\left(\frac{d\phi}{dn}\right)_2 - \lambda_1\left(\frac{d\phi}{dn}\right)_1 = -(\lambda_2 X'_2 - \lambda_1 X'_1)\cos n,x \\ -(\lambda_2 Y'_2 - \lambda_1 Y'_1)\cos n,y - (\lambda_2 Z'_2 - \lambda_1 Z'_1)\cos n,z, \end{cases}$$

and hence at the boundary between a conductor and a non-conductor the form

$$(15_h)\quad \frac{d\phi}{dn} = -X'\cos n,x - Y'\cos n,y - Z'\cos n,z.$$

In addition to these limiting conditions we have, according to section (8), at limiting surfaces where the electromotive forces become infinite, the further condition

$$(15_i)\begin{cases} \phi_1 - \phi_2 = \int(X\cos n,x + Y\cos n,y + Z\cos n,z)dn, \\ \quad = \int(X'\cos n,x + Y'\cos n,y + Z'\cos n,z)dn, \\ \quad = \phi_{1,2}. \end{cases}$$

These conditions together determine ϕ definitely within a constant which remains dependent upon the conditions outside the conductor. For homogeneous conductors the equations (15_f) to (15_i) assume the simpler forms—

$$(15_k)\begin{cases} \Delta\phi = 0 \text{ for the interior of the conductor,} \\ \lambda_1\left(\frac{d\phi}{dn}\right)_1 = \lambda_2\left(\frac{d\phi}{dn}\right)_2 \text{ for the boundary between two conductors,} \\ \frac{d\phi}{dn} = 0 \text{ for the boundary adjoining a non-conductor,} \\ \phi_1 - \phi_2 = \phi_{1,2} \text{ at a bounding surface where electromotive effects occur.} \end{cases}$$

The equations thus obtained admit of immediate application to problems on current-distribution in bodies of three dimensions. Their application to lamellar conductors or to linear conductors is easy, and gives the definition of resistance, Ohm's law for closed circuits, Kirchhoff's laws for branched circuits, as well as the other laws relating to the distribution of steady currents.

16. *Magnetic Forces of Steady Currents*

In order to determine the forces L, M, N produced by the current-components u, v, w, which are now known, we introduce as subsidiary magnitudes the so-called components of the vector-potential, putting

$$U = \int \frac{u}{r} d\tau, \qquad V = \int \frac{v}{r} d\tau, \qquad W = \int \frac{w}{r} d\tau.$$

The integrals are to be extended over the whole space; thus it follows from the conditions of the steady state that

$$\frac{dU}{dx} + \frac{dV}{dy} + \frac{dW}{dz} = 0.$$

We now put

$$(16_a) \quad \begin{cases} L = A\left(\frac{dV}{dz} - \frac{dW}{dy}\right), & M = A\left(\frac{dW}{dx} - \frac{dU}{dz}\right), \\ N = A\left(\frac{dU}{dy} - \frac{dV}{dx}\right). \end{cases}$$

These quantities L, M, N are solutions of equations (15_b), and satisfy the equation

$$\frac{dL}{dx} + \frac{dM}{dy} + \frac{dN}{dz} = 0.$$

If, therefore, the forces actually present differ from these, the differences between the two still satisfy the conditions for the forces of statical magnetism, and may be regarded as arising from these latter; this, however, does not exclude the supposition that the magnetism itself is due to currents. But if no statical magnetism is present at all, the formulæ above given represent completely the magnetic forces present.

If we have only to deal with linear conductors, in which the current i flows, then the expressions $ud\tau$, $vd\tau$, $wd\tau$ in the quantities U, V, W are replaced by the expressions idx, idy, idz, where dx, dy, dz are the projections of the element ds of the circuit on the three axes; and the integrations must then be taken round all the circuits. Suppose we wish to regard the magnetic forces of the whole current as the sum of the actions of the separate current-elements. In order

to simplify the formulæ, let us suppose the element to be at the origin and the point $x'\ y'\ z'$ to be in the xy-plane; then an analysis of our integrals, which, as far as its results are concerned, is admissible, gives for the action of the current-element idx upon the point $x'\ y'\ z'$—

$$\mathrm{L}=0, \quad \mathrm{M}=0, \quad \mathrm{N}=\mathrm{A}idx\frac{d\frac{1}{r}}{dy'}=-\frac{\mathrm{A}idx}{r^2}\cdot\frac{y'}{r},$$

which formulæ contain the expression of Ampère's rule and the Biot-Savart law.

Wherever u, v, w vanish, *i.e.* everywhere outside the conductor in which the current flows, the values of the forces must in accordance with equations (15_b) possess a potential Ψ, to whose negative differential coefficients we can equate them. If the forces arise from only a single closed linear circuit, this potential can be expressed in the form

$$(16_b) \quad \Psi=-\mathrm{A}i\int\frac{d\frac{1}{r}}{dn}d\omega+\text{const.}$$

where $d\omega$ denotes the element of any surface through the circuit, n the normal to this surface, and where the integration is extended over the whole of the surface bounded by the circuit. We here regard as positive that side of the surface from which the current appears to flow in the direction in which the hands of a clock move. For the negative differential coefficients of the above expression can in all cases be brought, by known methods of transforming integrals, into the forms found for L, M, N. Except in the circuit itself these differential coefficients are therefore everywhere finite and continuous; and, even if the integral contained in Ψ becomes discontinuous at the surface ω, the necessary continuity can always be conferred upon Ψ itself if we regard the constant contained in it as having an infinite number of values, and employ a value varying by $4\pi\mathrm{A}i$ whenever we pass through the surface ω. The potential itself thus attains an infinite number of values, and changes in value by $4\pi\mathrm{A}i$ each time we return to the same point after passing round the circuit.

Various interpretations can be assigned to the integral

which occurs in Ψ. In the first place, it can be regarded as the potential due to a magnetic shell. By following out this conception we arrive at Ampère's theory of magnetism. Again we may, with Gauss, regard the value of this integral at a given point as the spherical angle which the circuit subtends at this point. From this, by an easy transition, we arrive at the following statement:—For any given point this integral represents the number of lines of force which proceed from an unit pole placed at the point and are embraced by the circuit. We may supplement this by applying the following statement to the potential itself (including its manifoldness):—The difference between its values at two points is equal to the product of Ai into the number of lines of force which cut the circuit in a definite direction when an unit pole is moved in any manner from the one point to the other.

From our standpoint the last interpretation is the most suitable; it also allows us, with the aid of sections (12) and (14), to deduce the following conclusions:—Firstly, the mechanical work which must be done in moving a magnet-pole, or a system of unchangeable magnetism, in the neighbourhood of a current whose strength is kept constant, is equal to the number of lines of force of the magnet-pole or magnetic system which cut the circuit in a definite direction, multiplied by the current and the constant A. Secondly, the mechanical work which must be done in moving a constant current in a magnetic field is equal to the number of lines of force which are cut by the circuit during the motion, multiplied by the current and the constant A. Lastly, and in particular, the mechanical work which must be done in moving a constant current 1 in the neighbourhood of a constant current 2, is equal to the number of lines of force proceeding from the circuit 2 which are cut by the circuit 1 during the motion, multiplied by the current in 1 and by the constant A. It is also equal to the number of lines of force proceeding from the circuit 1 which cut the circuit 2 during the motion, multiplied by the current in 2 and by the constant A. Both expressions lead to the same result; we can prove this by representing the product of the current in the one circuit and the number of lines of force from the other circuit which pass through it, by an expression which is symmetrical with reference to both. For let the

symbols i, ds refer to the circuit 1; and the symbols i', ds', U', V', W', L', M', N' refer to the circuit 2. Then the product of Ai into the number of lines of force from 2 which pass through 1 is equal to

$$\begin{aligned}
&\mathrm{A}i\int(\mathrm{L}'\cos n,x+\mathrm{M}'\cos n,y+\mathrm{N}'\cos n,z)d\omega\\
&=\mathrm{A}^2i\int\Big\{\Big(\frac{d\mathrm{V}'}{dz}-\frac{d\mathrm{W}'}{dy}\Big)\cos n,x+\Big(\frac{d\mathrm{W}'}{dx}-\frac{d\mathrm{U}'}{dz}\Big)\cos n,y\\
&\qquad\qquad+\Big(\frac{d\mathrm{U}'}{dy}-\frac{d\mathrm{V}'}{dx}\Big)\cos n,z\Big\}\,d\omega\\
&=-\mathrm{A}^2i\int(\mathrm{U}'\cos s,x+\mathrm{V}'\cos s,y+\mathrm{W}'\cos s,z)ds\\
&=-\mathrm{A}^2ii'\iint\frac{\cos s,x\cos s',x+\cos s,y\cos s',y+\cos s,z\cos s',z}{r}\,dsds'\\
&=-\mathrm{A}^2ii'\iint\frac{\cos\epsilon}{r}\,dsds',
\end{aligned}$$

where ϵ denotes the angle between the two current-elements. The expression obtained is symmetrical with respect to both circuits. We know that in fact the variations of this expression—Neumann's potential of the one circuit upon the other multiplied by A$^2ii'$—gives the work required for the relative displacement of closed currents, and hence the ponderomotive forces which exist between the currents when at rest. We also know that this statement contains everything that can with certainty be asserted respecting the ponderomotive forces which arise between currents.

We shall next calculate the magnetic energy of a space in which the stationary current-components u, v, w and the unchangeable magnetic densities m are distributed, assuming the restriction that no magnetisable bodies are present in the space. If Ψ now represents the potential of the magnetisms m, we obtain the energy successively in the forms—

$$(16_c)\quad\left\{\begin{aligned}
&\frac{1}{8\pi}\int(\mathrm{L}^2+\mathrm{M}^2+\mathrm{N}^2)d\tau\\
&=\frac{\mathrm{A}}{8\pi}\int\Big\{\mathrm{L}\Big(\frac{d\mathrm{V}}{dz}-\frac{d\mathrm{W}}{dy}-\frac{1}{\mathrm{A}}\frac{d\Psi}{dx}\Big)+\mathrm{M}\Big(\frac{d\mathrm{W}}{dx}-\frac{d\mathrm{U}}{dz}-\frac{1}{\mathrm{A}}\frac{d\Psi}{dy}\Big)\\
&\qquad\qquad+\mathrm{N}\Big(\frac{d\mathrm{U}}{dy}-\frac{d\mathrm{V}}{dx}-\frac{1}{\mathrm{A}}\frac{d\Psi}{dz}\Big)\Big\}d\tau
\end{aligned}\right.$$

$$(16_c)\quad \begin{cases} = \dfrac{A}{8\pi}\int\left\{U\left(\dfrac{dM}{dz}-\dfrac{dN}{dy}\right)+V\left(\dfrac{dN}{dx}-\dfrac{dL}{dz}\right)\right. \\ \qquad\qquad \left. +W\left(\dfrac{dL}{dy}-\dfrac{dM}{dx}\right)\right\}d\tau \\ \qquad +\dfrac{1}{8\pi}\int\Psi\left(\dfrac{dL}{dx}+\dfrac{dM}{dy}+\dfrac{dN}{dz}\right)d\tau \\ = \frac{1}{2}A^2\int(Uu+Vv+Ww)d\tau+\frac{1}{2}\int\Psi m d\tau, \\ \text{or, in the case of linear currents—} \\ = \frac{1}{2}A^2\int\int\dfrac{ii'\cos\epsilon}{r}dsds'+\frac{1}{2}\int\Psi m d\tau, \end{cases}$$

where, in the first part of the last form, the integration is to be carried out with respect to both ds and ds', and is to include all currents present. It is clear from this last form that the displacement of unchangeable magnets with respect to unchangeable currents does not alter the magnetic energy of the space. Hence the mechanical work which is done in such a displacement does not find its compensation in the variation of the magnetic energy of the space, as it does in the case of the displacement of unchangeable magnets among themselves; we must account in some other way for the work which has been done. It further appears from the same formula that the relative displacement of currents which are maintained constant does determine a change in the energy of the space, which is equal to the absolute value of the work done. But when we pay due regard to the signs, we see that this change does not take place in such a sense that it can be regarded as the compensation for the lost mechanical energy, but in the opposite sense. Here again, then, we have to account for double the amount of work which the mechanical forces do in the relative displacement of the circuits. We shall return to this at the end of the following section.

Dynamical Phenomena

From among the infinite number of possible forms of the variable state, comparatively few groups of phenomena have hitherto fallen under observation. We shall refer to these groups without attempting any exhaustive and systematic classification of the subject.

17. *Induction in Closed Circuits*

In accordance with equations (9_a) electric forces must necessarily be present in a varying magnetic field. In general these forces must be very weak, for they contain the very small factor A; on this account they can only be detected through the currents which they excite in closed circuits, or through their cumulative action in very long linear circuits which are closed to within a small fraction of their lengths. Hence the effects which can be experimentally measured invariably give us only the integral effect of the electric force in a closed circuit, *i.e.* the integral $\int(X dx + Y dy + Z dz)$ taken along a looped line. According to a known method of transforming integrals, which we have already used, this line-integral is equal to the surface-integral

$$\int\left\{\left(\frac{dZ}{dy}-\frac{dY}{dz}\right)\cos n,x+\left(\frac{dX}{dz}-\frac{dZ}{dx}\right)\cos n,y+\left(\frac{dY}{dx}-\frac{dX}{dy}\right)\cos n,z\right\}d\omega,$$

taken over any surface ω bounded by the line in question. Applying equations (9_a) this expression becomes equal to

$$A\frac{d}{dt}\int(\mathfrak{L}\cos n,x+\mathfrak{M}\cos n,y+\mathfrak{N}\cos n,z)d\omega.$$

We may express this in words as follows:—The electromotive force which manifests itself in a closed circuit is equal to the variation per unit time of the number of magnetic lines of force which traverse the circuit multiplied by A. In particular, if the induction arises from a closed variable current, and if it is assumed that no magnetisable bodies are present, then according to the results of the previous section the induced electromotive force is equal to the product of the Neumann's potential of the two circuits on one another and the variation per unit time of the inducing current, multiplied by A^2. These laws—of which the first is the more general—with their consequences embrace all the phenomena of induction which have been actually observed in the case of conductors at rest.

Induction in moving conductors lies beyond the range to which the present dissertation is restricted. But as far as linear conductors are concerned, the transition from the case of

induction in conductors at rest can be made by the following statement:—Whether the magnetic field in the immediate neighbourhood of a closed circuit changes in consequence of the motion of ponderable bodies, or in consequence of purely electromagnetic changes of state, the electromotive force produced in the closed circuit is the same, provided the change in the magnetic field in its immediate neighbourhood is the same. In accordance with this and the previous statements, the induced electric force in a conductor in motion is equal to the number of lines of force which are cut by the conductor in a definite direction per unit of time, multiplied by A. The product of this electric force and of the current in the moving conductor gives, according to section (11), the thermal or chemical work done by induction in the conductor. It follows from the results of the preceding section, if we pay due regard to sign, that this is equal to the mechanical work which must be done by the external forces acting upon the circuit. Hence, if a current of constant strength is maintained in a circuit, and this circuit is moved towards a fixed magnet, the thermal and chemical energy developed in the circuit accounts for the mechanical work done; while the magnetic energy of the system remains constant. But, on the other hand, if this circuit is moved towards another in which a constant current is maintained, the larger amount of thermal and chemical energy developed in the one through the motion accounts for the mechanical work done; and the same extra amount of energy which appears in the other circuit accounts for the diminution in the magnetic energy of the field. Or, to speak more accurately, the sum of the former amounts of energy balances the sum of the latter. This settles the point referred to at the end of section (16).

18. *Electromagnetics of Unclosed Currents*

With regard to the phenomena which are possible, this is the richest region of all; for it includes all those problems which we cannot apportion elsewhere as special cases. But as far as actual experience is concerned, it is a region which hitherto has been but slightly explored. The oscillations of unclosed induction-circuits, or of discharging Leyden jars, can be

treated with sufficient approximation according to the laws of the preceding section; and so far only the electric waves and oscillations of short wave-length, which have been discussed in the earlier papers, strictly belong here. With regard to the theoretical treatment of this section we must therefore refer to these earlier papers—pointing out, however, that the splitting up of the electric force into an electrostatic and an electromagnetic part does not in these general problems convey any physical meaning which can be clearly conceived, nor is it of any great mathematical use; so that, instead of following earlier methods of treatment, it will be expedient to avoid it.

19. *Optical Phenomena in Isotropic Bodies*

We include in optics those electromagnetic disturbances which are purely periodic in time, and whose period does not exceed a very small fraction, say the billionth (10^{-12}) part, of a second. By none of the means which are at our disposal for detecting such disturbances can we recognise the magnetic and electric forces as such; what we are able to detect are simply the geometrical relations according to which the existing disturbance is propagated in different directions with different intensities. Hence the mathematical representation of the phenomena may also be confined to following the propagation of one of the two kinds of force, after eliminating the opposite kind; and it is immaterial which of the two is chosen for consideration. If we restrict ourselves to homogeneous isotropic non-conductors and eliminate in the one case the electric, in the other the magnetic, force-components, we obtain from equations (4_a) and (4_b) the following equations:—

$$(19_a)\quad \begin{cases} A^2\epsilon\mu\dfrac{d^2L}{dt^2} = \Delta L, \\ A^2\epsilon\mu\dfrac{d^2M}{dt^2} = \Delta M, \\ A^2\epsilon\mu\dfrac{d^2N}{dt^2} = \Delta N, \\ \dfrac{dL}{dx}+\dfrac{dM}{dy}+\dfrac{dN}{dz} = 0, \end{cases} \qquad (19_b)\quad \begin{cases} A^2\epsilon\mu\dfrac{d^2X}{dt^2} = \Delta X, \\ A^2\epsilon\mu\dfrac{d^2Y}{dt^2} = \Delta Y, \\ A^2\epsilon\mu\dfrac{d^2Z}{dt^2} = \Delta Z, \\ \dfrac{dX}{dx}+\dfrac{dY}{dy}+\dfrac{dZ}{dz} = 0. \end{cases}$$

The solutions of these, assuming that the disturbances are purely periodic, are always solutions of the equations (4_a) and (4_b) as well. From each of the two systems of equations (19_a) and (19_b) it can be seen that transverse waves are possible, and that longitudinal waves are impossible; each of the two systems gives for the velocity of the possible waves the value

$$1/A\sqrt{\epsilon\mu};$$

from each of the two systems the phenomena of rectilinear propagation, of diffraction, of the interference of natural and of polarised light can be deduced, and the different kinds of polarisation can be understood. By returning to equations (4_a) and (4_b) it can be shown that the simultaneous directions of the electric and the magnetic force at any point of a plane wave are invariably perpendicular to one another.

Suppose that the surface of separation of two homogeneous isotropic bodies coincides with the xy-plane. In accordance with section (8), and bearing in mind that we are dealing only with periodic disturbances, the following conditions obtain at this surface of separation :—

$$(19_c)\begin{cases} L_1 = L_2, \\ M_1 = M_2, \\ \mu_1 N_1 = \mu_2 N_2; \end{cases} \qquad (19_d)\begin{cases} X_1 = X_2, \\ Y_1 = Y_2, \\ \epsilon_1 Z_1 = \epsilon_2 Z_2. \end{cases}$$

Each of these systems of limiting equations, together with the corresponding equations for the interior of both bodies, gives the laws of reflection, of refraction, of total reflection,—in fact, the fundamental laws of geometrical optics. From each of them it follows that the intensity of reflected and refracted waves is dependent upon the nature of their polarisation, and that this dependence, as well as the retardation of phase of the totally reflected waves, is in accordance with Fresnel's formulæ. If we deduce these formulæ from the equations of the electric forces (19_b) and (19_d), it will be found that the method of development corresponds with the method of deducing these formulæ as given by Fresnel himself. If we start from the equations of the magnetic force (19_a) and (19_c), we approach the path by which F. Neumann arrived at Fresnel's equations. From our more general

standpoint we cannot only see from the start that both paths must lead to the same goal, but we can also recognise that the two are equally satisfactory. In the actually observed phenomena of reflection the electric and magnetic forces are not completely interchangeable, and the two paths appear to be different. This is because the magnetic permeabilities are almost the same and equal to unity for all bodies which come under consideration, whereas the specific inductive capacities differ considerably; and hence the optical behaviour of bodies is chiefly determined by their electrical properties.

If the xy-plane forms the boundary between our non-conductor and a perfect conductor, the following equations obtain in this plane :—

$$(19_e) \qquad N = 0,$$
$$(19_f) \quad X = 0, \quad Y = 0.$$

From these, together with the corresponding equations for the interior of the non-conductor, it follows that for every angle of incidence and every azimuth of polarisation the reflection is total. Since all actual conductors occupy an intermediate position between perfect conductors and non-conductors, the reflection from them may be expected to be of a kind intermediate between total reflection and the reflection from transparent bodies. Inasmuch as metallic reflection occupies such a position, our equations appear adapted for giving a general picture of metallic reflection as well. Up to the present, however, investigation does not enable us to state how far such a representation, by suitable choice of the constants, can be extended into details.

It has already been pointed out in the first section that the phenomena of dispersion require the introduction of at least two electric or two magnetic quantities, and that they therefore lie outside the limits of our present theory.

20. *Optics of Crystalline Bodies*

We shall confine our attention to optical phenomena in the interior of a homogeneous, completely transparent crystal,—in which we further assume that the axes of symmetry of the

electric and the magnetic energy coincide. Let the co-ordinate axes be parallel to these common axes of symmetry, and, for the sake of simplicity, let us write

$$\epsilon_1, \epsilon_2, \epsilon_3, \mu_1, \mu_2, \mu_3, \text{ instead of } \epsilon_{11}, \epsilon_{22}, \epsilon_{33}, \mu_{11}, \mu_{22}, \mu_{33}.$$

Equations (5_a) and (5_b), which here come under consideration, now take the form

$$(20_a)\begin{cases} A\mu_1 \dfrac{dL}{dt} = \dfrac{dZ}{dy} - \dfrac{dY}{dz}, \\ A\mu_2 \dfrac{dM}{dt} = \dfrac{dX}{dz} - \dfrac{dZ}{dx}, \\ A\mu_3 \dfrac{dN}{dt} = \dfrac{dY}{dx} - \dfrac{dX}{dy}; \end{cases} \qquad (20_b)\begin{cases} A\epsilon_1 \dfrac{dX}{dt} = \dfrac{dM}{dz} - \dfrac{dN}{dy}, \\ A\epsilon_2 \dfrac{dY}{dt} = \dfrac{dN}{dx} - \dfrac{dL}{dz}, \\ A\epsilon_3 \dfrac{dZ}{dt} = \dfrac{dL}{dy} - \dfrac{dM}{dx}. \end{cases}$$

These equations are integrated by assuming that the light consists of plane waves of plane-polarised light, corresponding to the following statements:—The magnetic force is perpendicular to the electric polarisation, and the electric force is perpendicular to the magnetic polarisation. In general the direction of both forces does not coincide with the wave-plane; the direction of both polarisations lies in the wave-plane. Hence the direction which is perpendicular to both polarisations is the wave-normal; the direction which is perpendicular to both forces is the direction in which, according to section (11), the energy is propagated; in optics it is called the ray. To every given position of the wave-normal there correspond in general two possible waves of different polarisations, different velocities, and different positions of the corresponding rays. If we suppose that at any given instant plane waves starting from the origin of co-ordinates proceed outwards in all possible directions of the wave-normals, these wave-planes after unit time envelop a surface,—the so-called wave-surface. Each single wave-plane touches the wave-surface at a point on the corresponding ray from the origin. The equation to the surface enveloped by the wave-planes is found to be

$$(20_c)\begin{aligned} &\left(\frac{x^2}{\epsilon_1}+\frac{y^2}{\epsilon_2}+\frac{z^2}{\epsilon_3}\right)\left(\frac{x^2}{\mu_1}+\frac{y^2}{\mu_2}+\frac{z^2}{\mu_3}\right) - \frac{x^2}{\epsilon_1\mu_1}\left(\frac{1}{\epsilon_2\mu_3}+\frac{1}{\epsilon_3\mu_2}\right) \\ &- \frac{y^2}{\epsilon_2\mu_2}\left(\frac{1}{\epsilon_1\mu_3}+\frac{1}{\epsilon_3\mu_1}\right) - \frac{z^2}{\epsilon_3\mu_3}\left(\frac{1}{\epsilon_1\mu_2}+\frac{1}{\epsilon_2\mu_1}\right) + \frac{1}{\epsilon_1\epsilon_2\epsilon_3\mu_1\mu_2\mu_3} = 0. \end{aligned}$$

The surface of the fourth degree represented by this equation cuts each of the co-ordinate planes in two ellipses. In one of the co-ordinate planes the two ellipses intersect each other in four points—the four conical points (*Nabelpunkte*) of the surface; in the two other co-ordinate planes one of the ellipses surrounds the other; and these statements hold good whatever the values of ϵ and μ are. To a very near approximation $\mu_1 = \mu_2 = \mu_3 = 1$ for all actual crystals; in this case the general form of the equation reduces to that of Fresnel's wave-surface, and of the two ellipses in which the surface cuts the co-ordinate planes, one reduces to a circle.

It is well known that the explanation of double refraction, of reflection at crystalline surfaces, and many of the interference-phenomena observed in crystals are intimately connected with the consideration of the wave-surface and the simpler forms which it assumes in special cases. But other facts, again, in crystallographic optics cannot be mastered by following out the idea of a single electric and a single magnetic directed magnitude; hence these facts lie outside the present limits of our theory.

In sections (17) to (20) we have completed the enumeration of those cases of the variable state whose importance has up to the present time given rise to the development of special theories.

XIV

ON THE FUNDAMENTAL EQUATIONS OF ELECTRO-MAGNETICS FOR BODIES IN MOTION

(*Wiedemann's Ann.* **41**, p. 369, 1890.)

AN account which I recently published[1] of electromagnetic processes in bodies at rest agreed, as far as the matter was concerned, with Maxwell's theory, but as far as the manner was concerned it aimed at a more systematic arrangement. From the outset the conception was insisted upon, that the electric and magnetic forces at any point owe their action to the particular condition of the medium which fills the space at that point; and that the causes which determine the existence and variations of these conditions are to be wholly sought in the conditions of the immediate neighbourhood, excluding all actions-at-a-distance. It was further assumed that the electric and magnetic state of the medium which fills space could be completely determined for every point by a single directed magnitude for each state; and it was shown that the restriction which lies in this assumption only excluded from consideration comparatively unimportant phenomena. The introduction of potentials into the fundamental equations was avoided.

The question now arises whether, while adhering strictly to the same views and the same limitations, the theory can be extended so as to embrace the course of electromagnetic phenomena in bodies which are in motion. We remark, in the first place, that whenever in ordinary speech we speak of bodies in motion, we have in mind the motion of ponderable matter alone. According to our view, however, the disturbances of the ether, which simultaneously arise, cannot be without effect;

[1] See XIII., p. 195.

and of these we have no knowledge. This is equivalent to saying that the question here raised cannot at present be treated at all without introducing arbitrary assumptions as to the motion of the ether. Furthermore, the few existing indications as to the nature of the motion of the ether lead us to suppose that the question above raised is strictly to be answered in the negative, for it appears to follow from such indications as we have, that even in the interior of tangible matter the ether moves independently of it; indeed, this view can scarcely be avoided in view of the fact that we cannot remove the ether from any closed space. If now we wish to adapt our theory to this view, we have to regard the electromagnetic conditions of the ether and of the tangible matter at every point in space as being in a certain sense independent of each other. Electromagnetic phenomena in bodies in motion would then belong to that class of phenomena which cannot be satisfactorily treated without the introduction of at least two directed magnitudes for the electric and two for the magnetic state.

But the state of the case is different if we explicitly content ourselves with representing electromagnetic phenomena in a narrower sense—up to the extent to which they have hitherto been satisfactorily investigated. We may assert that among the phenomena so embraced there is not one which requires the admission of a motion of the ether independently of ponderable matter within this latter; this follows at once from the fact that from this class of phenomena no hint has been obtained as to the magnitude of the relative displacement. At least this class of electric and magnetic phenomena must be compatible with the view that no such displacement occurs, but that the ether which is hypothetically assumed to exist in the interior of ponderable matter only moves with it. This view includes the possibility of taking into consideration at every point in space the condition of only one medium filling the space; and it thus admits of the question being answered in the affirmative. For the purpose of the present paper we adopt this view. It is true that a theory built on such a foundation will not possess the advantage of giving to every question that may be raised the correct answer, or even of giving only one definite answer; but it at

least gives possible answers to every question that may be propounded, *i.e.* answers which are not inconsistent with the observed phenomena nor yet with the views which we have obtained as to bodies at rest.

We therefore assume that at every point a single definite velocity can be assigned to the medium which fills space; and we denote the components of this in the directions of x, y, z, by α, β, γ. We regard these magnitudes as being everywhere finite, and treat them as varying continuously from point to point. Of course we also admit discontinuous variations, but we regard them as being only the limiting cases of very rapid continuous variations. We further limit each permissible discontinuity by the restriction that it shall in no case lead to the formation of empty spaces. The necessary and sufficient condition for this is that the three differential coefficients $d\alpha/dx$, $d\beta/dy$, $d\gamma/dz$ should everywhere be finite. Wherever we find tangible matter in space we definitely deduce the values of α, β, γ from the motion of this. Wherever we do not find in the space any tangible matter, we may assign to α, β, γ any arbitrary value which is consistent with the given motions at the boundary of the empty space, and is of the same order of magnitude. We might, for example, give α, β, γ those values which would exist in the ether if it moved like any gas. We further use all the symbols which occur in the preceding paper in the same sense here. We here regard electric and magnetic force as signs of the condition of the moving matter in the same sense in which we have hitherto regarded them as signs of the conditions of matter at rest. Electric and magnetic polarisation we simply regard as a second and equivalent means of indicating the same conditions. We also assign to the lines of force, by which we represent these polarisations, precisely the same meaning.

1. *Statement of the Fundamental Equations for Bodies in Motion*

At any point of a body at rest the time-variation of the magnetic state is determined simply by the distribution of the electric force in the neighbourhood of the point. In the case of a body in motion there is, in addition to this, a second

variation which at every instant is superposed upon the first, and which arises from the distortion which the neighbourhood of the point under consideration experiences through the motion. We now assert that the influence of the motion is of such a kind that, if it alone were at work, it would carry the magnetic lines of force with the matter. Or more precisely:—Supposing that at any given instant the magnetic state of the substance was represented in magnitude and direction by a system of lines of force; then a system of lines of force passing through the same material points would also represent in magnitude and direction the magnetic state at any other time, if the effect of the motion alone had to be considered. The corresponding statement holds good for the variation which the electric polarisation experiences through the motion. These statements suffice for extending to moving bodies the theory already developed for bodies at rest; they clearly satisfy the conditions which our system of itself requires, and it will be shown that they embrace all the observed facts.[1]

In order to represent our ideas symbolically, let us first, during the time-element dt, fix our attention upon a small surface-element in the interior of the moving matter, which at the beginning of this time-element lies parallel to the yz-plane, and during the motion is displaced and distorted with the matter. We distribute and draw the magnetic lines of force so that the number of them which penetrates the surface-element at the beginning of the time dt is $\mathfrak{L}$. Everywhere and always $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ will then denote the number of lines of force which traverse a surface-element of equal area parallel to the yz, xz, xy-planes respectively. The number of lines of force which traverses our particular surface-element now varies owing to several causes; we shall consider separately the amount which each separate cause contributes. In the first place, the number would vary even if the surface-element remained in its original position; this variation amounts to $(d\mathfrak{L}/dt)dt$, if $d\mathfrak{L}/dt$ denotes the rate of variation of $\mathfrak{L}$ at a point which, with reference to our system of co-ordinates, is at rest. In the second place, since the surface-element is displaced with the velocity α, β, γ to places where other values

[1] [See Note 33 at end of book.]

of $\mathfrak{L}$ obtain, the rate of variation due to this cause amounts to $(ad\mathfrak{L}/dx + \beta d\mathfrak{L}/dy + \gamma d\mathfrak{L}/dz)dt$. In the third place, the plane of the element rotates with velocity da/dy about the z-axis, and with velocity da/dz about the y-axis, and lines of force will be embraced by the element which originally were parallel to it; the amount due to this cause is—$(\mathfrak{M}da/dy + \mathfrak{N}da/dz)dt$. Finally the surface of the element increases with velocity $d\beta/dy + d\gamma/dz$; and for this cause the number increases by an amount $\mathfrak{L}(d\beta/dy + d\gamma/dz)dt$. If the sum of these quantities is equal to zero, there can be no change in the number; we have therefore reckoned up completely all causes of variation, and since all the amounts are very small, their sum represents the total variation. We may also analyse the total variation in another manner which has a more distinct physical significance, viz. into the amount which the presence of the electric forces in the neighbourhood, and the amount which the motion would contribute, each by itself and in the supposed absence of the other cause. According to the laws which hold good for conductors at rest, the first amounts to $(dZ/dy - dY/dz)dt \,.\, 1/A$; according to the statement which we have just made, the latter is zero; the first of itself represents the total variation. We equate the two expressions found for the total variation, divide by dt, multiply by A, add and subtract the terms $ad\mathfrak{M}/dy + ad\mathfrak{N}/dz$, rearrange the terms and thus obtain, after treating similarly the other components of the magnetic force and the components of the electric force, the following system of fundamental equations for bodies in motion:—

$$(1_a)\quad \begin{cases} A\left\{\dfrac{d\mathfrak{L}}{dt} + \dfrac{d}{dy}(\beta\mathfrak{L} - a\mathfrak{M}) - \dfrac{d}{dz}(a\mathfrak{N} - \gamma\mathfrak{L}) + a\left(\dfrac{d\mathfrak{L}}{dx} + \dfrac{d\mathfrak{M}}{dy} + \dfrac{d\mathfrak{N}}{dz}\right)\right\} \\ \qquad = \dfrac{dZ}{dy} - \dfrac{dY}{dz}, \\ A\left\{\dfrac{d\mathfrak{M}}{dt} + \dfrac{d}{dz}(\gamma\mathfrak{M} - \beta\mathfrak{N}) - \dfrac{d}{dx}(\beta\mathfrak{L} - a\mathfrak{M}) + \beta\left(\dfrac{d\mathfrak{L}}{dx} + \dfrac{d\mathfrak{M}}{dy} + \dfrac{d\mathfrak{N}}{dz}\right)\right\} \\ \qquad = \dfrac{dX}{dz} - \dfrac{dZ}{dx}, \\ A\left\{\dfrac{d\mathfrak{N}}{dt} + \dfrac{d}{dx}(a\mathfrak{N} - \gamma\mathfrak{L}) - \dfrac{d}{dy}(\gamma\mathfrak{M} - \beta\mathfrak{N}) + \gamma\left(\dfrac{d\mathfrak{L}}{dx} + \dfrac{d\mathfrak{M}}{dy} + \dfrac{d\mathfrak{N}}{dz}\right)\right\} \\ \qquad = \dfrac{dY}{dx} - \dfrac{dX}{dy}, \end{cases}$$

$$(1_b)\quad \begin{cases} \mathrm{A}\left\{\dfrac{d\mathfrak{X}}{dt}+\dfrac{d}{dy}(\beta\mathfrak{X}-a\mathfrak{N})-\dfrac{d}{dz}(a\mathfrak{Z}-\gamma\mathfrak{X})+a\left(\dfrac{d\mathfrak{X}}{dx}+\dfrac{d\mathfrak{N}}{dy}+\dfrac{d\mathfrak{Z}}{dz}\right)\right\} \\ \qquad =\dfrac{d\mathrm{M}}{dz}-\dfrac{d\mathrm{N}}{dy}-4\pi\mathrm{A}u, \\ \mathrm{A}\left\{\dfrac{d\mathfrak{N}}{dt}+\dfrac{d}{dz}(\gamma\mathfrak{N}-\beta\mathfrak{Z})-\dfrac{d}{dx}(\beta\mathfrak{X}-a\mathfrak{N})+\beta\left(\dfrac{d\mathfrak{X}}{dx}+\dfrac{d\mathfrak{N}}{dy}+\dfrac{d\mathfrak{Z}}{dz}\right)\right\} \\ \qquad =\dfrac{d\mathrm{N}}{dx}-\dfrac{d\mathrm{L}}{dz}-4\pi\mathrm{A}v, \\ \mathrm{A}\left\{\dfrac{d\mathfrak{Z}}{dt}+\dfrac{d}{dx}(a\mathfrak{Z}-\gamma\mathfrak{X})-\dfrac{d}{dy}(\gamma\mathfrak{N}-\beta\mathfrak{Z})+\gamma\left(\dfrac{d\mathfrak{X}}{dx}+\dfrac{d\mathfrak{N}}{dy}+\dfrac{d\mathfrak{Z}}{dz}\right)\right\} \\ \qquad =\dfrac{d\mathrm{L}}{dy}-\dfrac{d\mathrm{M}}{dx}4\pi\mathrm{A}w, \end{cases}$$

which are completed by the linear relations which connect the polarisations and the current-components with the forces. The constants of these relations are to be regarded as functions of the varying conditions of the moving matter, and to this extent as functions of the time as well.[1]

Our method of deducing the equations (1_a) and (1_b) does not require that the system of co-ordinates used should remain absolutely fixed in space. We can, therefore, without change of form, transform our equations from the system of co-ordinates first chosen to a system of co-ordinates moving in any manner through space, by taking a, β, γ to represent the velocity-components with reference to the new system of co-ordinates, and referring the constants ϵ, μ, λ, X', Y', Z', which depend upon direction, at every instant to these. From this it follows that the absolute motion of a rigid system of bodies has no effect upon any internal electromagnetic processes whatever in it, provided that *all* the bodies under consideration, including the ether as well, actually share the motion. It further follows from this consideration that even if only a single part of a moving system moves as a rigid body, the processes which occur in this part follow exactly the same course as in bodies at rest. If, nevertheless, the existing motion does exert any influence upon this part, this influence can only arise in those portions of the system in which distortion of the elements occurs, and must be propagated thence into those portions which move

[1] [See Note 34 at end of book.]

after the manner of rigid bodies. If, for example, a solid mass of metal is suddenly displaced in the magnetic field, then, according to our equations, the only direct or simultaneous effect of this disturbance is upon the surface and the neighbourhood of the metallic mass; it here gives rise to electric forces which afterwards produce secondary effects—penetrating into the interior of the mass and giving rise to currents in it.

The equations here stated are in form and intention closely related to those by which von Helmholtz in vol. lxxviii. of Borchardt's *Journal* represented the behaviour of the electric and magnetic forces in moving bodies.[1] From this source the notation is partly borrowed. And yet our equations differ from those given by v. Helmholtz not only in form, but also in meaning, at least with regard to such members as have not hitherto been tested by experiment. Maxwell himself does not seem to me to have aimed in his treatise at any systematic treatment of the phenomena in moving bodies.[2] The numerous references which he makes to such phenomena are either confined to approximations, or relate only to cases which do not involve any necessary distinction between the theories of direct and of indirect action.

2. *The Physical Meaning of the Separate Terms*

Equations (1_a) and (1_b) tell us the future value of the polarisations at every fixed point in space or, if we prefer it, in each element of the moving matter, as a definite and determinate consequence of the present electromagnetic state and the present motion in the neighbourhood of the point under consideration. This is the physical meaning of them in accordance with the conception which our system represents. The common conception of the relations expressed by these equations is quite different. It regards the rates of variation of the polarisations on the left-hand side as the cause, and the induced forces on the right-hand side as the

[1] v. Helmholtz, *Ges. Abhandl.* 1, p. 745; Borchardt's *Journ. f. Mathem.* 78, p. 273, 1874.

[2] [This statement is not quite correct. It does indeed hold good for Maxwell's treatise, to which it refers; but in his paper "On Physical Lines of Force" (*Phil. Mag.*, April 1861) Maxwell has himself given a complete and systematic treatment of the phenomena in moving bodies. Unfortunately I had not noticed this when writing my paper.]

consequence thereof. This conception has arisen through the fact that the polarisations and their variations are usually sooner and more clearly known to us than the forces which simultaneously arise; so that, as far as our knowledge goes, the left-hand sides of the equations are prior to the right-hand sides. In the cases which chiefly interest us this conception has indeed very great advantages; but from the general standpoint it has the disadvantage that the forces are not definitely determined by the rates of variation of the polarisations of the opposite kind, but contain terms which are independent of these variations. The common theory gets out of this difficulty by setting these terms as electrostatic or magnetic forces in opposition to the electromagnetic forces which are alone, according to that theory, determined by our equations. Although we do not approve of such a separation, and hence do not accept the common conception as to the causal relationship, it is still interesting to show how the partial forces which are introduced in the usual theory are contained in the separate terms of our equations. For this purpose we split up the forces in the form $X = X_1 + X_2$, etc., $L = L_1 + L_2$, etc., and put—

$$(2)\begin{cases} X_1 = A(\gamma\mathfrak{M} - \beta\mathfrak{N}), & L_1 = A(\beta\mathfrak{Z} - \gamma\mathfrak{Y}), \\ Y_1 = A(a\mathfrak{N} - \gamma\mathfrak{L}), & M_1 = A(\gamma\mathfrak{X} - a\mathfrak{Z}), \\ Z_1 = A(\beta\mathfrak{L} - a\mathfrak{M}), & N_1 = A(a\mathfrak{Y} - \beta\mathfrak{X}). \end{cases}$$

We thus obtain for X_2, Y_2, Z_2, L_2, M_2, N_2 equations which result from the equations (1_a) and (1_b) for X, Y, Z, L, M, N by omitting the second and third terms on the left-hand side. Now the resultant of X_1 Y_1 Z_1 is an electric force which arises as soon as a body moves in the magnetic field. It is perpendicular to the direction of the motion and to the direction of the magnetic lines of force; it is that force which in a narrower sense we are accustomed to denote as the electromotive force induced through motion. But it should be observed that, according to our views, the separation of this from the total force can have no physical meaning; for it would be in opposition to our conception to suppose that the magnetic field within a body could have a motion relative to it. The counterpart to the force X_1 Y_1 Z_1 is the force L_1 M_1 N_1, which must make itself felt in a non-conductor when the

latter is displaced through the lines of force of an electric field; but this is not yet confirmed by experience and is absent from the older electromagnetics.

Let us now turn our attention to the resultant of $L_2\ M_2\ N_2$, and suppose the general solutions of the equations containing these quantities to be represented as functions of the quantities

$$u,\ d\mathfrak{X}/dt,\ a(d\mathfrak{X}/dx + d\mathfrak{Y}/dy + d\mathfrak{Z}/dz),\ \text{etc.}$$

Let us put these latter quantities in the functions all equal to zero; there still remains a first part of the force which does not owe its origin to electromagnetic causes. Its components necessarily possess a potential; it represents that distance-force which, according to the older view, proceeds from magnetic masses. A second part of the force is given by that part of the functions which vanishes when, and only when, u, v, w vanish. It contains the magnetic distance-force which appears to proceed from the actual electric currents. We obtain the whole of the electromagnetic part of the force $L_2\ M_2\ N_2$ by replacing in the expression of the second part the quantity $4\pi Au$ by the quantity

$$4\pi Au + A\frac{d\mathfrak{X}}{dt} + Aa\left(\frac{d\mathfrak{X}}{dx} + \frac{d\mathfrak{Y}}{dy} + \frac{d\mathfrak{Z}}{dz}\right)$$

and treating v and w similarly. This corresponds to the statement that as far as the production of a magnetic distance-force is concerned, an actual current is to be regarded as equivalent in the first place to the variation of an electric polarisation, and in the second place to the convective motion of true electricity. The latter part of this statement finds its requisite confirmation in Rowland's experiment.

Finally, let us consider the force $X_2\ Y_2\ Z_2$. We can separate from this force as well a part which is independent of time-variations of the system, which possesses a potential, and which is treated as an electrostatic distance-force. From the residue of the electromagnetic force which remains we can detach a second part, which vanishes when, and only when, the quantities $d\mathfrak{L}/dt$, $d\mathfrak{M}/dt$, $d\mathfrak{N}/dt$ vanish. It clearly represents the force of induction which arises from varying magnetic moments, but it also contains in a hidden form that electric force which owes its origin to varying currents.

Finally, there remains a third and last part which can be interpreted as an electric force produced by a convective motion of magnetism, and in which must be found the explanation of certain known phenomena of unipolar induction.

These considerations show that we might also have arrived at the system of equations (1_a) and (1_b) by summing up the effects of the separate forces required by the older theories, and adding a series of hypothetical terms which can neither be confirmed nor disproved by existing experience. The way which we have followed requires a smaller number of independent hypotheses. We now proceed to deduce from our equations the most important general results.

3. *Motion of Magnets and of Electrostatically Charged Bodies*

As independent causes of variation of the electric or magnetic polarisation there appear in our scheme first the magnetic or electric forces respectively, and secondly the motion of material bodies. According to our conclusions in the case of bodies at rest, the first cause produces no displacement of true electricity in non-conductors and no displacement of true magnetism at all. The latter cause of itself produces a displacement of electricity and of magnetism towards the space at rest, but it can cause no displacement towards the matter in motion; for by its motion this matter carries with it the lines of force, and electricity and magnetism may be regarded as the free ends of these lines. Hence when both causes act together there can be no relative motion of true magnetism with reference to the surrounding matter; nor can there be any such relative motion of true electricity, at any rate in non-conductors. Under these circumstances electricity and magnetism move with the matter in which they are present, as if they were indestructible and adhered firmly to the parts thereof. In order to represent this same idea symbolically, let us differentiate first the equations (1_a) and then the equations (1_b) with respect to $x\,y\,z$, multiply by the volume-element $d\tau$ which we suppose to remain at rest, and to which the quantities $\mathfrak{L}$, $\mathfrak{M}$, etc., refer. Let $d\tau'$ denote a volume-element which at every instant encloses the matter contained at the present instant in $d\tau$; let de' and dm' denote the amounts of true elec-

tricity and true magnetism respectively contained in $d\tau'$, and $\mathfrak{L}'$, $\mathfrak{M}'$, etc., the values of $\mathfrak{L}$, $\mathfrak{M}$, etc., with reference to $d\tau'$. We thus obtain—

$$(3_a)\quad \begin{cases} \left\{\frac{d}{dt}\left(\frac{d\mathfrak{L}}{dx}+\frac{d\mathfrak{M}}{dy}+\frac{d\mathfrak{N}}{dz}\right)\right. \\ \quad +\alpha\frac{d}{dx}\left(\frac{d\mathfrak{L}}{dx}+\frac{d\mathfrak{M}}{dy}+\frac{d\mathfrak{N}}{dz}\right)+\beta\frac{d}{dy}\left(\frac{d\mathfrak{L}}{dx}+\frac{d\mathfrak{M}}{dy}+\frac{d\mathfrak{N}}{dz}\right) \\ \qquad\qquad +\gamma\frac{d}{dz}\left(\frac{d\mathfrak{L}}{dx}+\frac{d\mathfrak{M}}{dy}+\frac{d\mathfrak{N}}{dz}\right) \\ \quad \left.+\left(\frac{d\alpha}{dx}+\frac{d\beta}{dy}+\frac{d\gamma}{dz}\right)\left(\frac{d\mathfrak{L}}{dx}+\frac{d\mathfrak{M}}{dy}+\frac{d\mathfrak{N}}{dz}\right)\right\}d\tau \\ \quad =\frac{d}{dt}\left\{\left(\frac{d\mathfrak{L}'}{dx}+\frac{d\mathfrak{M}'}{dy}+\frac{d\mathfrak{N}'}{dz}\right)d\tau'\right\}=4\pi\frac{dm'}{dt}=0, \end{cases}$$

$$(3_b)\quad \begin{cases} \left\{\frac{d}{dt}\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)\right. \\ \quad +\alpha\frac{d}{dx}\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)+\beta\frac{d}{dy}\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right) \\ \qquad\qquad +\gamma\frac{d}{dz}\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right) \\ \quad \left.+\left(\frac{d\alpha}{dx}+\frac{d\beta}{dy}+\frac{d\gamma}{dz}\right)\left(\frac{d\mathfrak{X}}{dx}+\frac{d\mathfrak{Y}}{dy}+\frac{d\mathfrak{Z}}{dz}\right)\right\}d\tau \\ \quad =\frac{d}{dt}\left\{\left(\frac{d\mathfrak{X}'}{dx}+\frac{d\mathfrak{Y}'}{dy}+\frac{d\mathfrak{Z}'}{dz}\right)d\tau'\right\}=4\pi\frac{de}{dt} \\ \quad =4\pi\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right). \end{cases}$$

These equations embrace the statements already made, and complete them as far as conductors are concerned. If the velocities $\alpha\ \beta\ \gamma$ are so small that the electric and magnetic conditions may at each instant remain infinitely near to the stationary state, and if we restrict ourselves to the consideration of such quasi-stationary states, then the results which we have obtained are sufficient and necessary to determine the interdependence of the various states which may arise from each other. The introduction of these results into such problems enables us to replace the complete, but very complicated, equations (1_a) and (1_b) by the equivalent and very simple equations which hold

good for statical problems in bodies at rest, and which can be deduced from equations (1_a) and (1_b) by equating to zero the velocities and the time-variations at all points of the space. Such a simplification of the statements is not possible without introducing the idea of electricity and of magnetism; and it seems to me that this is the principal reason why these ideas are indispensable in the study of electrostatics and in the representation of magnetic phenomena.

4. *Induction in Closed Circuits*

The greatest velocities which we can assign to the surrounding bodies are so small compared with the velocity of light—the reciprocal of which appears as the multiplier of a, β, γ in equations (1_a) and (1_b)—that electromagnetic effects due purely to motion can only be investigated with precision in the particular case in which these effects consist in the induction of an electric current in a closed metallic conductor. In order to determine the magnitude of such effects in closed conductors, let us consider any unclosed portion ω of a surface in the interior of the matter under consideration, and which is displaced with the material particles during the motion. Let s represent the instantaneous limiting curve of this surface-element. Let ζ' denote the number of magnetic lines of force which at any time traverse the surface ω. We shall again consider the causes which produce (independently of each other) a variation in ζ' as being two—in the first place, the electric forces; and in the second place, the motion of matter. If the first cause alone were at work, the system would be at rest, and so the rate of variation of ζ' multiplied by A would be equal to the integral of the electric force taken around the whole extent of s; the integral, viewed from the side of the positive normal, being taken clock-wise. If the motion alone were at work, it would not produce any variation of ζ', for it would carry forward the lines of force traversing the surface ω together with this surface itself. Hence in the actual case in which the two causes act together, the integral of the electric force taken in the given sense around any closed curve s is equal to A multiplied by the rate of variation of the number of mag-

netic lines of force which traverse any surface which was originally bounded by the curve s, but which follows the motion. This law also holds good for the special case—the only one which is important from an experimental point of view—in which the curve s follows the path of a linear conductor; nor does it become invalid when the motion is sufficiently slow to allow all the states which arise to appear as being steady, and the current as uniform in all parts of the conductor.

To represent this symbolically, let $n'x$, $n'y$, $n'z$ denote the angle which the normal to the element $d\dot{\omega}$ of the moving surface $\dot{\omega}$ makes at any instant with the axes. Let $\mathfrak{L}'\ \mathfrak{M}'\ \mathfrak{N}'$ be the values of $\mathfrak{L}\ \mathfrak{M}\ \mathfrak{N}$ in this element. Further, let $d\omega$, n,x, n,y, n,z denote the values of $d\dot{\omega}$, $n'x$, $n'y$, $n'z$, in the original position. We observe that, from purely geometrical considerations, we have

$$\frac{d}{dt}(d\dot{\omega}\cos n'x) = d\omega\left\{\left(\frac{d\beta}{dy}+\frac{d\gamma}{dz}\right)\cos n,x - \frac{d\beta}{dx}\cos n,y - \frac{d\gamma}{dx}\cos n,z\right\},$$

$$\frac{d}{dt}(d\dot{\omega}\cos n'y) = d\omega\left\{-\frac{da}{dy}\cos n,x + \left(\frac{da}{dx}+\frac{d\gamma}{dz}\right)\cos n,y - \frac{d\gamma}{dy}\cos n,z\right\},$$

$$\frac{d}{dt}(d\dot{\omega}\cos n'z) = d\omega\left\{-\frac{da}{dz}\cos n,x - \frac{d\beta}{dz}\cos n,y + \left(\frac{da}{dx}+\frac{d\beta}{dy}\right)\cos n,z\right\},$$

and we thus obtain

$$\frac{d\zeta'}{dt} = \frac{d}{dt}\int(\mathfrak{L}'\cos n'x + \mathfrak{M}'\cos n'y + \mathfrak{N}'\cos n'z)d\dot{\omega}$$

$$= \int\left(\frac{d\mathfrak{L}}{dt}+a\frac{d\mathfrak{L}}{dx}+\beta\frac{d\mathfrak{L}}{dy}+\gamma\frac{d\mathfrak{L}}{dz}\right)\cos n,x\,d\omega$$

$$+\int\left(\frac{d\mathfrak{M}}{dt}+a\frac{d\mathfrak{M}}{dx}+\beta\frac{d\mathfrak{M}}{dy}+\gamma\frac{d\mathfrak{M}}{dz}\right)\cos n,y\,d\omega$$

$$+\int\left(\frac{d\mathfrak{N}}{dt}+a\frac{d\mathfrak{N}}{dx}+\beta\frac{d\mathfrak{N}}{dy}+\gamma\frac{d\mathfrak{N}}{dz}\right)\cos n,z\,d\omega$$

$$+\int\mathfrak{L}\left(\frac{d\beta}{dy}+\frac{d\gamma}{dz}\right)\cos n,x\,d\omega - \int\mathfrak{L}\frac{d\beta}{dx}\cos n,y\,d\omega - \int\mathfrak{L}\frac{d\gamma}{dx}\cos n,z\,d\omega$$

$$-\int\mathfrak{M}\frac{da}{dy}\cos n,x\,d\omega - \int\mathfrak{M}\left(\frac{da}{dx}+\frac{d\gamma}{dz}\right)\cos n,y\,d\omega - \int\mathfrak{M}\frac{d\gamma}{dy}\cos n,z\,d\omega$$

$$-\int\mathfrak{N}\frac{da}{dz}\cos n,x\,d\omega - \int\mathfrak{N}\frac{d\beta}{dz}\cos n,y\,d\omega + \int\mathfrak{N}\left(\frac{da}{dx}+\frac{d\beta}{dy}\right)\cos n,z\,d\omega;$$

from which, with the aid of equations (1_a) and (1_b), we get

$$A\frac{d\zeta'}{dt} = \int\left\{\left(\frac{dZ}{dy} - \frac{dY}{dz}\right)\cos n,z + \left(\frac{dX}{dz} - \frac{dZ}{dx}\right)\cos n,y + \left(\frac{dY}{dx} - \frac{dX}{dy}\right)\cos n,z\right\}d\omega$$
$$= \int(Xdz + Ydy + Zdz),$$

the last integral being taken around the whole extent s of the surface $d\omega$.

In special cases these results admit of simplification. If it is possible to shut off a singly connected space which entirely contains the moving curve s, and in which there is no true magnetism, it is clearly immaterial whether the auxiliary surface $\dot{\omega}$ follows the motion of the material parts or suffers a displacement independently of these, provided it remains within the space referred to, and is bounded by the curve s. In this case we may more simply and yet definitely assert that the integral of the electric force taken around the closed curve is equal to the time-rate of variation of the number of magnetic lines of force embraced by the curve s, multiplied by A. If we retain this same supposition, and if in addition the magnetic polarisation at every fixed point of the space is constant in spite of the motion of the curve s, we may assert that the induced force along the curve is equal to A multiplied by the number of magnetic lines of force, considered as at rest, which the curve s cuts in a given direction during its motion. If the magnetic forces, under the influence of which the curve s moves, are simply and solely due to the influence of an uniform current along a path t, then the number of lines of force traversing s is, as we have seen,[1] equal to the product of the Neumann's potential of the curves s and t, and of the current in t. In this case, therefore, the variation in the above-mentioned product per unit time multiplied by A gives the electromotive force acting along the curve s.

In one form or another these theorems embrace all known cases of induction which have been carefully investigated. The laws of unipolar induction, too, can be easily deduced from the general propositions. Quantitative investigations of induction-phenomena in bodies of three dimensions have only been carried out to a limited extent. The equations by which

[1] See p. 232.

Jochmann[1] and others have succeeded in representing the known facts, follow directly from our general equations by omitting a number of terms which naturally disappear in consequence of the special nature of the problem.

We must not omit to mention that we may represent the general theorem of induction in another and a very elegant form if we allow ourselves to speak of an independent motion of the lines of force, and to regard in general every variation of the magnetic polarisation as the result of such a motion of the lines of force. If we do this, we may state generally and completely that the induced electromotive force in any closed curve s is equal to the product of A into the number of lines of magnetic force which are cut by the curve s in a definite sense per unit time. But although no objection can be raised to the occasional use of the conception therein involved, nevertheless it will be better for us to avoid it in the present paper. For the conception employed by Faraday, and developed by Poynting,[2] of a motion of the lines of force relatively to the surrounding medium, is indeed a highly remarkable one, and may be capable of being worked out; but it is entirely different from the view here followed, according to which the lines of force simply represent a symbol for special conditions of matter. There is no meaning in speaking of an independent motion of such conditions. It should also be observed that the controllable decrease and increase of the lines of force in all parts of the space does not definitely determine the presupposed motion of the lines of force. Hence the above-mentioned proposition would not of itself decide definitely the magnitude of the induction in all cases; it should rather be regarded as a definition by means of which one among the possible motions of the lines of force is pointed out as the effective motion.

5. *Treatment of Surfaces of Slip*

At the boundary of two heterogeneous bodies the electromagnetic constants may pass from one value to another discontinuously; but the velocity-components $\alpha\beta\gamma$ do not

[1] Jochmann, *Crelle's Journ.* **63**, p. 1, 1863.

[2] J. H. Poynting, *Phil. Trans.* **2**, p. 277, 1885. [See also Note 35 at end of book.]

necessarily undergo discontinuous changes at the same time at this bounding surface. The surfaces of separation between solid bodies and fluids, or between fluids themselves, are to be regarded as surfaces of discontinuity of this kind; and we are free to suppose that the transition at the boundary between a body and the ether is of the same nature. The appearance of continuous motion at such surfaces of discontinuity does not give rise to any new considerations; the conditions of the material parts on both sides of the surface are connected by the same relations as those which obtain for bodies at rest.

But the case is different when the velocity-components also undergo discontinuous variations at the surface. As observed in our Introduction, the discontinuity can only refer to the components of the velocity which are parallel to the surface of separation; we therefore rightly denote surfaces of this kind as surfaces of slip (*Gleitflächen*). They may exist between solid bodies which are in contact with one another; it is also occasionally convenient and—seeing how ignorant we are as to the actual circumstances—permissible, to regard the surface of separation between a body and the ether as a surface of slip. As we have already remarked in the Introduction, we treat a surface of slip as the limiting case of a transition-layer in which the motions, and possibly the electromagnetic constants as well, change very rapidly, but still continuously from one value to another. This conception is justified by the fact that it does not lead to any results in contradiction with experience; and it enables us to assert that the general propositions which we have already deduced do not become invalid in a system in which there are surfaces of slip. In order that our conception may suffice to determine the conditions in the surface of separation, the nature of the transition must be subjected to certain general restrictions. We give these restrictions in the form of hypotheses respecting the finiteness of certain magnitudes in the transition-layer itself. We assume that there are no electromotive forces at the surface of slip. We place the origin of the system of co-ordinates to which we refer at any point of the element of the transition-layer under consideration, and let it also follow this point during the motion. We further give the z-axis such a direction that it stands perpendicular to the element of the surface of slip, and also

remains perpendicular during the motion. Thus the transition-layer always forms the immediate neighbourhood of the xy-plane. We assume that even in the transition-layer itself the quantities

$$\begin{array}{cccccc} X & Y & Z & L & M & N \\ \mathfrak{X} & \mathfrak{Y} & \mathfrak{Z} & \mathfrak{L} & \mathfrak{M} & \mathfrak{N} \\ u & v & w & \alpha & \beta & \gamma \end{array}$$

remain finite; and in the same way that the differential coefficients of these quantities parallel to the surface of slip, *i.e.* with respect to x and y, and also the differential coefficients of the quantities

$$\mathfrak{X} \quad \mathfrak{Y} \quad \mathfrak{Z} \quad \mathfrak{L} \quad \mathfrak{M} \quad \mathfrak{N}$$

with reference to the time t, remain finite. On the other hand, we should allow the differential coefficients with respect to z to become infinite, with the exception of $d\gamma/dz$, which, in accordance of the remark in the Introduction already referred to, must remain finite. Everywhere in the transition-layer, accordingly, γ itself is vanishingly small. These assumptions being made, we multiply the first two equations of the system (1_a) and (1_b) by dz, integrate with respect to $\bullet$ through the transition-layer between two points lying exceedingly near to it, and observe that, on account of the shortness of the integration-path, the integral of every quantity which remains finite in the layer vanishes. We thus obtain the following four equations, in which the index 1 refers to the one side, the index 2 to the other side, of the surface of separation—

$$(5_a) \quad A\int_1^2 \mathfrak{N}\frac{da}{dz}dz = Y_2 - Y_1, \quad -A\int_1^2 \mathfrak{N}\frac{d\beta}{dz}dz = X_2 - X_1;$$

$$(5_b) \quad -A\int_1^2 \mathfrak{Z}\frac{da}{dz}dz = M_2 - M_1, \quad A\int_1^2 \mathfrak{Z}\frac{d\beta}{dz}dz = L_2 - L_1.$$

These equations give the mutual relations between the force-components tangential to the surface of separation on both sides of it. Here, as in the case of bodies at rest, the components normal to the surface are connected by the condition that the surface-density of the true magnetism at the surface of separation

cannot alter excepting by convection, and that the surface-density of the true electricity can only alter either by convection or by an actual current.

If the element of the surface of separation under consideration is not charged with any true electricity or true magnetism, $\mathfrak{Z}$ and $\mathfrak{N}$ are constant in the interior of the transition-layer. In this case the equations (5_a) and (5_b) take the simpler forms

$$(5_c) \quad X_2 - X_1 = A\mathfrak{N}(\beta_1 - \beta_2), \qquad Y_2 - Y_1 = A\mathfrak{N}(\alpha_2 - \alpha_1),$$
$$(5_d) \quad L_2 - L_1 = A\mathfrak{Z}(\beta_2 - \beta_1), \qquad M_2 - M_1 = A\mathfrak{Z}(\alpha_1 - \alpha_2).$$

As an example of the application of these equations, let us consider the case of a solid of revolution rotating about its axis within a hollow in another solid body which closely surrounds it. If this system is submitted to the action of a magnetic field which is symmetrical with reference to the axis of rotation, there will not be, according to our conception, either in the interior of the rotating body, or in the interior of the surrounding mass, any cause for the appearance of electric forces. Such forces are, in fact, absent when the magnetic excitement is entirely restricted to the interior of the one body or of the other. But if the lines of force penetrate through the surface along which the two bodies slide past one another, the electromotive forces expressed by equation (5_c) are excited at this surface; these forces spread into the interior of the bodies and there produce the electric stresses and currents whose existence is shown by experiment. If the bodies under consideration are non-conductors and are subjected to the influence of electric forces which are distributed symmetrically with reference to the axis of rotation, and which do not vanish at the surface of slip, the introduction of the motion excites magnetic forces in the neighbourhood in accordance with equation (5_d). It is true that effects of this kind cannot be observed with the same certainty as those first referred to; but there is at least an indication of them in Professor Röntgen's experiments.[1]

In the general case in which there are charges of true electricity and true magnetism at the surface of separation, a knowledge of the surface-density of these is not by itself sufficient for ascertaining the integrals of the equations (5_a)

[1] W. C. Röntgen, *Wied. Ann.* **35**, p. 264, 1888.

and (5_b); beyond this it is necessary to know to what extent the electricity and magnetism in the transition-layer share in the motion of each of the two contiguous bodies. This indeterminateness lies in the very nature of the matter. Consider, for example, Rowland's experiment on the effect of the convective motion of electricity; and suppose the electrified disc to rotate within a solid insulator surrounding it closely, instead of rotating in air. Clearly the magnetic effect would diminish, even to the point of vanishing entirely, as the electricity gradually escaped from the surface of the rotating disc on to the contiguous surface of the body at rest.

6. *Conservation of Energy—Ponderomotive Forces*

We shall consider the transition of the system from the initial to the final state during any element of time as being split up into two stages. In the first stage we shall suppose all the material parts to be transferred from their initial to their final position, the lines of force simply following the motion of the material parts. In the second stage we shall suppose that the electric and magnetic forces, which by this time are present, come into action, and in turn transfer the electromagnetic conditions into their final state. The variation which the electromagnetic energy of the system experiences during the whole period of transition is the sum of the variations which it experiences during the two stages. The processes which take place during the second stage are processes in bodies at rest; we already know how the variations of the electromagnetic energy during such processes are compensated by other forms of energy. But during the first stage, too, the electromagnetic energy of each material part of the system alters; we have therefore to account for what becomes of the electromagnetic energy thus diminished, or to find the source of any increase. As far as all existing experience extends, it can be proved beyond doubt that in every self-contained electromagnetic system the amount of energy in question is balanced by the mechanical work which is done by the electric and magnetic ponderomotive forces of the system during the element of time under consideration. But, nevertheless, taken as a statement of general applicability, this is

not sufficient to enable us to deduce generally and rigidly the ponderomotive forces from the calculable variations of the electromagnetic energy. For this reason we introduce two further assumptions which are not inconsistent with it; these are not required by experience but by our own particular views. The first assumption declares that the statement already made—which experience proves to be correct for every self-contained electromagnetic system—also holds good for any material part of such a system. The second assumption asserts that no part of the system can exert upon the rest of the system any ponderomotive forces excepting pressures which are exerted by the elements of the first part upon the contiguous elements of the remaining part, and which at every point of the surface of contact depend simply upon the electromagnetic conditions of the immediate neighbourhood. The pressures required by the second assumption are determined without ambiguity by the first assumption; we shall deduce the magnitude of these pressures, and shall show that they are sufficient to explain the facts which have been directly observed. It then follows from the mode in which the pressures are deduced that the principle of the conservation of energy is satisfied in the case of moving bodies as well.

Consider during an element of time dt the magnetic energy of a material particle, whose varying volume may be denoted by $d\tau'$, while $d\tau$ denotes the value of $d\tau'$ at the beginning of the time-element dt. For the sake of simplicity let the origin of our system of co-ordinates be placed permanently in a material point of the space $d\tau'$. If $d\tau'$ moved as a rigid body, carrying its lines of force with it, the amount of energy contained in it would not alter. In general, therefore, the variation of this energy must be simply a function of the distortion which $d\tau'$ experiences in consequence of the motion; our immediate problem is to represent the variation in this form. Now it is not the polarisations alone which alter in consequence of the distortions, but also the properties of the material vehicles thereof, *i.e.* the magnetic constants. For the purpose of calculating this variation we need a further extension of our notation. In the first place, and in addition to the constants μ, we define a series of constants μ' by the condition that

$$\begin{aligned}&\mathfrak{L}L + \mathfrak{M}M + \mathfrak{N}N\\ &= \mu_{11}L_2 + 2\mu_{12}LM + \text{etc.}\\ &= \mu_{11}'\mathfrak{L}_2 + 2\mu_{12}'\mathfrak{L}\mathfrak{M} + \text{etc.}\end{aligned}$$

These constants μ' are therefore the coefficients of $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$ in the linear functions of these quantities by which the forces are represented. We further denote by $\xi\,\eta\,\zeta$ the displacements which the point, whose velocities are $\alpha\,\beta\,\gamma$, suffers from its original position at the beginning of the time dt. The quantities

$$\frac{d\xi}{dx} = x_x, \qquad \frac{d\xi}{dy} + \frac{d\eta}{dx} = x_y, \text{ etc.},^1$$

are then the components of the distortions of the element $d\tau'$ in which the displacements $\xi\,\eta\,\zeta$ occur. The constants μ' are functions of these quantities; moreover, they depend upon the rotations ρ, σ, τ which the element experiences during the distortion. During the element of time dt both x_x, x_y, etc., and ρ, σ, τ remain vanishingly small; hence the dependence is linear and is known to us, provided we are given the differential coefficients of μ' with respect to ρ, σ, τ, x_x, x_y, etc. The differential coefficients with respect to ρ, σ, τ can be calculated from the instantaneous values of μ' itself. But this is not possible for the differential coefficients with respect to x_x, x_y, etc., and we must therefore assume that we are otherwise given the quantities

$$\frac{d\mu_{11}'}{dx_x} = \mu_{11}', {}_{11}, \qquad \frac{d\mu_{11}'}{dx_y} = \mu_{11}', {}_{12}, \text{ etc.},$$

$$\frac{d\mu_{12}'}{dx_x} = \mu_{12}', {}_{11}, \qquad \frac{d\mu_{12}'}{dx_y} = \mu_{12}', {}_{12}, \text{ etc. etc.}$$

The 36 constants so defined clearly correspond to the magnetic properties of the particular substance which fills the space $d\tau'$ in its instantaneous state of deformation. For our purpose we cannot dispense with a single one of these constants; nor can we *a priori* deduce a single one of them from the magnetic properties of the substance which we have hitherto considered. By a suitable orientation of our system of co-

[1] Cf. G. Kirchhoff, *Mechanik*, p. 123, 1877.

ordinates we can reduce the number of necessary constants; similarly a reduction takes place when there happen to be symmetrical relations with respect to the system of co-ordinates used. In the simplest case, in which the substance is not only isotropic in its initial state, but also remains isotropic in spite of every deformation that arises—viz. in a fluid,—the number of the new constants reduces to a single one, which then, together with the one magnetic permeability, sufficiently defines the magnetic properties. Besides, it does not seem improbable that even in the general case necessary relations may be proved to exist between the constants which would then reduce to a smaller number of independent constants.

This notation being now assumed, we obtain successively the following expressions for the variation per unit time of the amount of magnetic energy contained in the space $d\tau'$:—

$$(6)\quad \begin{cases} \dfrac{d}{dt}\left\{\dfrac{1}{8\pi}(\mathfrak{L}\mathrm{L}+\mathfrak{M}\mathrm{M}+\mathfrak{N}\mathrm{N})d\tau'\right\} \\ \quad = \dfrac{1}{8\pi}\left\{d\tau\dfrac{d}{dt}(\mu_{11}'\mathfrak{L}^2+2\mu_{12}'\mathfrak{L}\mathfrak{M}+\text{etc.})\right. \\ \qquad \left. +(\mathfrak{L}\mathrm{L}+\mathfrak{M}\mathrm{M}+\mathfrak{N}\mathrm{N})\dfrac{d}{dt}d\tau'\right\} \\ \quad = \dfrac{1}{8\pi}d\tau\left\{2\left(\mathrm{L}\dfrac{d\mathfrak{L}}{dt}+\mathrm{M}\dfrac{d\mathfrak{M}}{dt}+\mathrm{N}\dfrac{d\mathfrak{N}}{dt}\right)\right. \\ \qquad +\left(\dfrac{d\mu_{11}'}{dt}\mathfrak{L}^2+2\dfrac{d\mu_{12}'}{dt}\mathfrak{L}\mathfrak{M}+\text{etc.}\right) \\ \qquad \left. +(\mathfrak{L}\mathrm{L}+\mathfrak{M}\mathrm{M}+\mathfrak{N}\mathrm{N})\left(\dfrac{da}{dx}+\dfrac{d\beta}{dy}+\dfrac{d\gamma}{dz}\right)\right\}. \end{cases}$$

From the last of these we proceed to remove the differential coefficients with respect to t. We obtain the following expressions for $d\mathfrak{L}/dt$, $d\mathfrak{M}/dt$, $d\mathfrak{N}/dt$ from equations (1_a) by considering only the influence of the motion in them, and putting the velocities α, β, γ, with due regard to the special choice of our co-ordinate system, equal to zero—

$$\frac{d\mathfrak{L}}{dt} = -\mathfrak{L}\left(\frac{d\beta}{dy}+\frac{d\gamma}{dz}\right)+\mathfrak{M}\frac{da}{dy}+\mathfrak{N}\frac{da}{dz},$$

$$\frac{d\mathfrak{M}}{dt} = -\mathfrak{M}\left(\frac{d\gamma}{dz}+\frac{da}{dx}\right)+\mathfrak{N}\frac{d\beta}{dz}+\mathfrak{L}\frac{d\beta}{dx},$$

$$\frac{d\mathfrak{N}}{dt} = -\mathfrak{N}\left(\frac{da}{dx}+\frac{d\beta}{dy}\right)+\mathfrak{L}\frac{d\gamma}{dx}+\mathfrak{M}\frac{d\gamma}{dy}.$$

For the magnitude $d\mu_{11}'/dt$ we further have

$$\begin{aligned}\frac{d\mu_{11}'}{dt} &= \frac{d\mu'_{11}}{dx_x}\cdot\frac{dx_x}{dt}+\frac{d\mu_{11}'}{dx_y}\cdot\frac{dx_y}{dt}+\text{etc.}\\ &\quad+\frac{d\mu_{11}'}{d\rho}\cdot\frac{d\rho}{dt}+\text{etc.}\\ &= \mu_{11}'_{,11}\frac{d\alpha}{dx}+\mu_{11}'_{,12}\left(\frac{d\alpha}{dy}+\frac{d\beta}{dx}\right)+\text{etc.}\\ &\quad+\tfrac{1}{2}\frac{d\mu_{11}'}{d\rho}\left(\frac{d\gamma}{dy}-\frac{d\beta}{dz}\right)+\text{etc.}\end{aligned}$$

We deduce similar expressions for $d\mu_{12}'/dt$, etc. We introduce all these expressions in the right-hand side of equation (6), and this side now becomes a homogeneous linear function of the nine differential coefficients of $\alpha\,\beta\,\gamma$ with respect to $x\,y\,z$. But we can and will arrange this function so that it shall appear to us as a homogeneous linear function of the six rates of deformation $d\alpha/dx$, $d\alpha/dy+d\beta/dx$, etc., and of the three rates of rotation $\frac{1}{2}(d\alpha/dy-d\beta/dx)$, etc. We here note that the coefficients of the three rates of rotation must necessarily vanish identically; for a motion of a particle as a rigid body does not bring about any alteration in the amount of energy contained in it. Accordingly, we simply reject the terms in which these rates of rotation occur, and thus obtain as our final result, after reducing to the unit of volume by dividing by $d\tau$—

$$\left.\begin{aligned}&\frac{1}{d\tau}\frac{d}{dt}\left\{\frac{1}{8\pi}(\mathfrak{L}\mathfrak{L}+\mathfrak{M}\mathfrak{M}+\mathfrak{N}\mathfrak{N})d\tau'\right\}\\ &=\frac{1}{8\pi}\frac{d\alpha}{dx}(\quad\mathfrak{L}\mathfrak{L}-\mathfrak{M}\mathfrak{M}-\mathfrak{N}\mathfrak{N}+\mu_{11}'_{,11}\mathfrak{L}^2+2\mu_{12}'_{,11}\mathfrak{L}\mathfrak{M}+\text{etc.})\\ &+\frac{1}{8\pi}\frac{d\beta}{dy}(-\mathfrak{L}\mathfrak{L}+\mathfrak{M}\mathfrak{M}-\mathfrak{N}\mathfrak{N}+\mu_{11}'_{,22}\mathfrak{L}^2+2\mu_{12}'_{,22}\mathfrak{L}\mathfrak{M}+\text{etc.})\\ &+\frac{1}{8\pi}\frac{d\gamma}{dz}(-\mathfrak{L}\mathfrak{L}-\mathfrak{M}\mathfrak{M}+\mathfrak{N}\mathfrak{N}+\mu_{11}'_{,33}\mathfrak{L}^2+2\mu_{12}'_{,33}\mathfrak{L}\mathfrak{M}+\text{etc.})\\ &+\frac{1}{8\pi}\left(\frac{d\beta}{dz}+\frac{d\gamma}{dy}\right)(\quad\mathfrak{N}\mathfrak{M}\quad+\quad\mathfrak{M}\mathfrak{N}+\mu_{11}'_{,23}\mathfrak{L}^2+2\mu_{12}'_{,23}\mathfrak{L}\mathfrak{M}+\text{etc.})\\ &+\frac{1}{8\pi}\left(\frac{d\gamma}{dx}+\frac{d\alpha}{dz}\right)(\quad\mathfrak{L}\mathfrak{N}\quad+\quad\mathfrak{N}\mathfrak{L}+\mu_{11}'_{,13}\mathfrak{L}^2+2\mu_{12}'_{,13}\mathfrak{L}\mathfrak{M}+\text{etc.})\\ &+\frac{1}{8\pi}\left(\frac{d\alpha}{dy}+\frac{d\beta}{dx}\right)(\quad\mathfrak{M}\mathfrak{L}\quad+\quad\mathfrak{L}\mathfrak{M}+\mu_{11}'_{,12}\mathfrak{L}^2+2\mu_{12}'_{,12}\mathfrak{L}\mathfrak{M}+\text{etc.})\end{aligned}\right\}\quad(6_a)$$

Now it is clear that in the linear function of the rates of distortion on the right hand the coefficient, taken negatively, of each of these rates, gives that pressure-component with which the magnetically strained matter tends to increase the corresponding distortion. For let us, in accordance with the usual[1] notation, denote by $X_x X_y X_z$ the components of the pressure which the matter of the element $d\tau$ exerts upon a plane section perpendicular to the x-axis; and let us further extend this notation to the directions of the other axes. Then the expression

$$X_x\frac{d\alpha}{dx}+Y_y\frac{d\beta}{dy}+Z_z\frac{d\gamma}{dz}$$
$$+Y_z\left(\frac{d\beta}{dz}+\frac{d\gamma}{dy}\right)+X_z\left(\frac{d\gamma}{dx}+\frac{d\alpha}{dz}\right)+X_y\left(\frac{d\alpha}{dy}+\frac{d\beta}{dx}\right)$$

represents the mechanical work, per unit volume and per unit time, done by the material contents of the element $d\tau$ in the distortion which takes place. According to our assumption this mechanical work is equal to the magnetic energy which is lost as a result of the distortion. Inasmuch as this holds for every possible deformation our assertion is shown to be correct. Each of the pressure-components obtained is a homogeneous quadratic function of the three components of the prevailing magnetic force or, similarly, of the three components of the prevailing magnetic polarisation. By exactly analogous considerations we can deduce exactly analogous expressions for the pressures which arise through electric stresses. The total pressure is equal to the sum of the magnetic and electric pressures.

Having now found the values of the ponderomotive pressures, we add three remarks. The first remark has reference to the difference between our system of pressures and the system which Maxwell has given for the general case in which the forces and the polarisations have different directions.[2] In the first place, Maxwell's formulæ are simpler, because in deducing them he paid no heed to the possible deformation of the medium. A much more important difference consists in the fact that the force-components which, according to the notation

[1] G. Kirchhoff (*Mechanik*, Eleventh Lecture).
[2] Maxwell, *Treatise on Elect. and Mag.* **2**, p. 254, 1873.

used, are denoted by X_y and Y_x, have different values in Maxwell, whereas with us they are identical. According to our system each material particle, when left to itself, simply changes its form; according to Maxwell's system it would at the same time begin to rotate as a whole. Hence Maxwell's pressures cannot owe their origin to processes in the interior of the element; and they therefore find no place in the theory here worked out. At the same time they are permissible, if one starts with the assumption that in the interior of the moving body the ether remains permanently at rest and provides the necessary point of support for the rotation which takes place.

The second remark has reference to the manner in which our formulæ become simplified when we apply them to bodies which are isotropic, and which, in spite of every deformation, remain isotropic — viz. to fluids. The system of constants μ' here reduces to the one constant $\mu' = 1/\mu$. If we further denote by σ the density of the fluid, we have

$$\mu_{11}{}', {}_{11} = \mu_{22}{}', {}_{22} = \mu_{33}{}', {}_{33} = -\frac{d\left(\frac{1}{\mu}\right)}{d\log\sigma} = \frac{1}{\mu^2}\frac{d\mu}{d\log\sigma},$$
$$\mu_{12}{}', {}_{11} = \text{etc.} = 0.$$

Thus the pressure-components are—

$$(6_b)\begin{cases} X_x = \frac{\mu}{8\pi}(-L^2+M^2+N^2) - \frac{d\mu}{8\pi d\log\sigma}(L^2+M^2+N^2), \\ Y_y = \frac{\mu}{8\pi}(\ L^2-M^2+N^2) - \frac{d\mu}{8\pi d\log\sigma}(L^2+M^2+N^2), \\ Z_z = \frac{\mu}{8\pi}(\ L^2+M^2-N^2) - \frac{d\mu}{8\pi d\log\sigma}(L^2+M^2+N^2), \end{cases}$$

$$X_y = -\frac{\mu}{4\pi}LM, \qquad X_z = -\frac{\mu}{4\pi}NL, \qquad Y_z = -\frac{\mu}{4\pi}MN.$$

For the same case quite identical formulæ have already been obtained by von Helmholtz [1] by following a similar train of thought. Our formulæ merge into his if we alter the notation so as to replace L, M, N and μ by λ/ϑ, μ/ϑ, ν/ϑ, and

[1] v. Helmholtz, *Wied. Ann.* 13, p. 400, 1881.

$1 + 4\pi\vartheta$, and further, notice that the θ of von Helmholtz's formulæ is equal to $d\vartheta/d \log \sigma = d\mu/4\pi d \log \sigma$.[1]

The third remark has reference to the question, in how far the resultants of the pressures deduced from our hypotheses are in accordance with the mechanical forces and couples, which we actually observe in bodies subjected to electromagnetic actions. We first observe that our observations are confined to systems which are indefinitely near to the statical or steady state. For such systems, however, the principle of the conservation of energy is of itself sufficient to enable us to calculate without ambiguity, from the loss of electromagnetic energy during any displacement, the magnitude of the opposing mechanical force-components; and it may be regarded as already proved that the force-components so calculated agree with those which are observed. One system of force-components which satisfies the principle of the conservation of energy is certainly given by the resultants of the pressures which have been deduced. This system must, therefore, be the one which is directly deducible from that principle, and which is in accordance with experience. In order to arrive *a posteriori* at the same result, we observe that under actual circumstances the electromagnetic pressures are much too feeble to cause noticeable deformations of the volume-elements of solid bodies. The very slight distortions which they are able to bring about we are accustomed, in the study of electricity, to treat separately as phenomena of electrostriction, apart from the phenomena of electromagnetics proper. If, therefore, we put on one side this special class of phenomena, it will not affect the result whether we admit the pressures as calculated by us in the case of solid bodies, or no pressures at all, or any others of the same order of magnitude. We may, therefore, in general content ourselves with the simpler formulæ (6_b), in which we are now to take μ to mean in the case of crystalline bodies any constant of the order of magnitude of μ_{11}, μ_{12}, etc. But we may further simplify the formulæ (6_b) by neglecting the terms which contain the quantity $d\mu/d \log \sigma$. For these terms, which represent an uniform pressure, cannot produce any finite displacements in liquids on

[1] The signs remain contrary, because v. Helmholtz reckons a tension as being positive, whereas we reckon a pressure as positive.

account of their small compressibility; they can only give rise to phenomena of electrostriction or magnetostriction respectively. In the case of gaseous bodies these terms disappear, for the constant μ and the specific inductive capacity do not change appreciably with the density σ. Those ponderomotive forces which produce finite relative displacements of the bodies must accordingly be represented by the resultants of the following pressure-system, which is to be regarded as acting everywhere:—

$$(6_c)\quad \begin{cases} X_x = \frac{\mu}{8\pi}(-L^2+M^2+N^2), \\ Y_y = \frac{\mu}{8\pi}(\ \ L^2-M^2+N^2), \\ Z_z = \frac{\mu}{8\pi}(\ \ L^2+M^2-N^2). \end{cases}$$

$$X_y = -\frac{\mu}{4\pi}LM, \qquad X_z = -\frac{\mu}{4\pi}NL, \qquad Y_z = -\frac{\mu}{4\pi}MN.$$

Now this simplified system of magnetic pressures is just Maxwell's.[1] Maxwell has shown that this, together with the corresponding electric system, embraces the observed ponderomotive forces between magnets, steady currents, and electrified bodies, and to his simple demonstration we may here refer.

It does not appear to have been observed that this system of pressures in general only leaves the interior of a homogeneous body, especially of the ether, at rest, if the acting forces possess a potential, *i.e.* if the prevailing conditions are statical or steady. In the case of any admissible electromagnetic disturbance, the pressures found must set the interior of the ether—which we have expressly supposed to be movable—into motion, with velocities which we could calculate if we had an idea of its mass.[2] This result seems to possess little intrinsic probability. And yet there is no reason, from the standpoint of the present dissertation, why we should abandon the theory on this account; for the result is not

[1] Maxwell, *Treatise on Elect. and Mag.*, 1873, **2**, p. 256. The signs there are opposite to ours, because Maxwell reckons a tension as being positive, whereas we reckon a pressure as positive.

[2] [See Note 36 at end of book.]

inconsistent with our hypotheses, nor yet with what experience teaches us. The small quantity of air which remains even in the highest vacua is amply sufficient to keep below perceptible magnitude all currents that could be excited in such spaces under the existing circumstances.

Finally, I wish to point out once more that I only attach value to the theory of electromagnetic forces in moving bodies here proposed from the point of view of systematic arrangement. The theory shows how we can treat completely the electromagnetic phenomena in moving bodies, under certain restrictions which we arbitrarily impose. It is scarcely probable that these restrictions correspond to the actual facts of the case. The correct theory should rather distinguish between the conditions of the ether at every point, and those of the embedded matter. But it seems to me that, in order to propound a theory in accordance with this view at present, we should require to make more numerous and arbitrary hypotheses than those of the theory here set forth.

ROYAL ASIATIC SOCIETY

SUPPLEMENTARY NOTES

(1891)

1. [II. p. 29.]

It was v. Helmholtz, in his paper *Ueber die Erhaltung der Kraft*, who first stated (in 1847) that the discharge of a Leyden jar is oscillatory. He arrived at this conclusion from its varying and opposite magnetic effects, and from the fact that when one endeavours to decompose water by electric discharges, both gases are developed at both electrodes. Sir William Thomson arrived independently at the same result from theoretical considerations. The mathematical treatment of the problem given by him in the year 1853 (*Phil. Mag.* (4) **5**, p. 393) still holds good to-day. We may further mention the following among the more important early papers on the subject:—

On the discharge of Leyden jars—

Feddersen, *Pogg. Ann.* **103**, p. 69, 1858; **108**, p. 497, 1859; **112**, p. 452, 1861; **113**, p. 437, 1861; **115**, p. 336, 1862; **116**, p. 132, 1862.

Paalzow, *Pogg. Ann.* **112**, p. 567, 1861; **118**, pp. 178, 357, 1863.

v. Oettingen, *Pogg. Ann.* **115**, p. 513, 1862; *Jubelbd.* p. 269, 1874.

G. Kirchhoff, *Pogg. Ann.* **121**, p. 551, 1864; *Ges. Abhandl.* p. 168.

L. Lorenz, *Wied. Ann.* **7**, p. 161, 1879.

On the oscillations of open induction-circuits—

Helmholtz, *Pogg. Ann.* **83**, p. 505, 1851; *Ges. Abhandl.* **1**, 429. The theory is implicitly contained in this, but is not explicitly applied to the special case of oscillations.

v. Helmholtz, *Ges. Abhandl.* **1**, p. 531, 1869.

Bernstein, *Pogg. Ann.* **142**, p. 54, 1871.

Schiller, *Pogg. Ann.* **152**, p. 535, 1872.

2. [II. p. 34.]

At first I insulated carefully with sealing-wax, etc. But I always found that, for all such experiments as are here considered,

the insulation afforded by dry wood is apply sufficient. In the subsequent experiments no other means of insulation was used.

3. [II. p. 39.]

I expect that the action of the induction-coil partly depends upon the fact that directly before the discharge it allows the potential to rise very rapidly. Several accessory phenomena lead me to believe that when this rapid rise takes place, the difference of potential is forced beyond the point at which sparking occurs when the difference of potential increases slowly; and that for this reason the discharge takes place more suddenly and energetically than when a statical charge is discharged.

4. [II. p. 45.]

These curves should be compared with the corresponding resonance-curves which Herr V. Bjerknes has obtained by more accurate experimental measurements (*Wied. Ann.* **44**, p. 74, 1891).

5. [II. p. 49.]

This remark in my first paper shows clearly that I never conceived the oscillations of my primary conductor as perfectly regular and long-continued sine-oscillations. The value of the damping has recently been carefully determined by Herr V. Bjerknes (*Wied. Ann.* **44**, p. 74, 1891). Fig. 40 shows, in accordance with the results of his experiments, the kind of oscillation given by a conductor similar to our primary conductor.

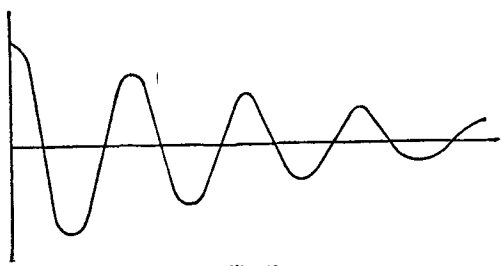


Fig. 40.

6. [II. p. 50.]

Just at this point there has crept into the calculation a fatal mistake, the unfortunate effects of which extend even to some of the subsequent papers.

The capacity C in the formula $T = \pi \sqrt{PC/A}$ denotes the amount of etectricity which exists at one end of an oscillating conductor when the difference of potential between the two ends is equal to unity. Now if these two ends consist of two spheres which are far apart from each other, and if their difference of potential is equal to unity, then the difference of potential between each of them and the surrounding space is equal to $\pm {}^1/_2$. Therefore the charge upon each of the spheres, measured in absolute units, is

found by dividing its capacity, *i.e.* its radius measured in centimetres, by 2. Hence we should here put $C = \frac{1}{2}^5$ cm., and not C = 15 cm. The period of oscillation, T, now becomes smaller in the proportion of $1 : \sqrt{2}$, so that T is now equal to 1·26 hundred-millionths of a second.

M. H. Poincaré, as already stated in the Introduction, first drew attention to this error (*Comptes Rendus*, **111**, p. 322, 1891).

7. [II. p. 52.]

The result is about right, but the way in which it is deduced is not sound. We have just referred [6] to an error in the calculation which would have to be corrected; and beside this no account is taken of damping through radiation. Indeed, I had not thought of this when writing the paper.

8. [IV. p. 67.]

The complications here mentioned, and the starting of long sparks by other much shorter ones, refer to the following phenomena:—Let the primary coils of two induction-coils be placed in the same circuit, and let their spark-gaps be so adjusted as to be just on the point of sparking. Any cause which starts sparking in one of them will now make the other begin to spark as well; and this quite independently of the mutual action of the light emitted by the two sparks—which, indeed, can easily be excluded. Sparking begins either in both, or else in neither of them. Again, let a Töpler-Holtz induction-machine, with a disc 40 cm. in diameter, be turned rapidly so as to give sparks having a maximum length of about 15 cm. Now draw the poles 20-25 cm. apart, so that the sparking entirely stops; it will now be found that a long crackling spark can again be regularly obtained every time a small spark is drawn from the negative conductor, either with the knuckle of the hand or with the knob of a Leyden jar; or the negative pole may be connected to a long conductor, and sparks may be drawn from this with the same result. The "releasing" spark may be quite short and weak; if it is drawn with the knob of a Leyden jar, the jar only appears slightly charged. The same effect cannot be obtained by drawing sparks from the positive pole. The phenomenon must have been often observed before; but I have not found any mention of it in the literature on the subject.

I can give no explanation of these phenomena. They clearly have the same origin as the phenomena which Herr G. Jaumann has described in his paper entitled "Einfluss rascher Potentialveränderungen auf den Entladungsvorgang" (*Sitzungsberichte d. Akad. d. Wissensch. zu Wien.* Bd. **97**, Abth. II*a*, July 1888). Herr Jaumann arrives at the conclusion that "not only the form, condition, and potential difference of the discharge-field," but also "the manner in which the potential difference alters, and probably its rate of

alteration, materially influence the discharge." It is to be hoped that these phenomena will be further explained.

9. [IV. p. 73.]

Soon afterwards Herren E. Wiedemann and H. Ebert showed that the action of the light only affects the negative pole, and only the surface of it (*Wied. Ann.* **33**, p. 241, 1888).

10. [IV. p. 76.]

Somewhat later I succeeded in this. I had hoped to observe an influence of the state of polarisation of the light upon the action, but was not able to detect anything of the kind.

11. [IV. p. 79.]

By this I did not mean to say that I had not succeeded in observing the action of light upon discharges other than those of induction-coils; but only that I had not succeeded in replacing spark-discharges—the nature of which is so little understood—by simpler means. This was first done by Herr Hallwachs (*Wied. Ann.* **33**, p. 301, 1888). The simplest effect that I obtained was with the glow-discharge from 1000 small Planté accumulators between brass knobs in free air; by the action of light I was able to make the glow-discharge pass when the knobs were so far apart that it could not spring across without the aid of the light.

12. [VII. p. 109.]

The 12 metres are supposed to be measured in the direction of the base-line. The space on each side of the base-line was clear up to a distance of 3·4 metres, with the exception of an iron stove which came within 1·5 metres of it. I did not think at the time that at this distance it could interfere at all.

13. [VII. p. 109.]

In this calculation as well the capacity is assumed to be that of an end-plate, supposed to be hanging free in air; this capacity was experimentally determined by comparison with the sphere previously used. For the reasons stated in Note 6, only the half of this capacity should have been taken. Hence the period of oscillation, as correctly calculated, is smaller than the value given in the proportion of $1 : \sqrt{2}$. Thus the correct value of the period of oscillation is almost exactly one hundred-millionth of a second.

14. [VII. p. 112.]

Here, as well as in all that follows, it is to be understood that, in order to produce stationary waves in wires, not only must the primary and secondary conductors be brought into resonance, but the straight stretched wires must also be tuned to unison with both of these. Only in this case does the whole length of the wire divide itself clearly into half wave-lengths, and only in this case is this beautiful phenomenon exhibited in its full development. This condition seems to have escaped the attention of some observers who have repeated the experiments on waves in wires.

15. [VII. p. 113.]

This has not turned out to be true. In tubes of about 2 cm. diameter, filled with dilute sulphuric acid, the waves travel quite well and with the same velocity as in wires. Herr E. Cohn has, moreover, shown that the inertia of the electrolytes cannot come into play when the period of oscillation is of the order here employed (*Wied. Ann.* **38**, p. 217). The fact that these oscillations are transmitted through electrolytes has been used by J. J. Thomson for the purpose of determining their resistance (*Proc. Roy. Soc.* **45**, p. 269).

16. [VII. p. 114.]

The correctly calculated period of oscillation is one hundred-millionth of a second. This, with a wave-length of 2·8 metres, gives a velocity of 280,000 km. per second, or approximately the velocity of light.

This is the final form,—although, of course, with much more careful data,—which Messrs. E. Lecher (*Wied. Ann.* **41**, p. 850) and Blondlot (*C. R.* **113**, p. 628) have adopted for showing that the velocity of waves in wires is the same as the velocity of light. As a matter of fact, however, this final form only shows the accordance of theory and observation in the following respect: that in a simple straight wire 2·8 metres long, and in a conductor of the form of our primary conductor, the periods of oscillation are equal. But the absolute value of the period of oscillation, and hence the velocity, might on that account differ by the same amount in both cases from the theoretical value; and it must differ by the same amount if the same causes produce equal retardations in both conductors.

Hence this final form cannot be employed for the purpose of removing doubts as to the existence of such a retardation.

The velocity assumed in the text depends much more upon the experiments of Fizeau and Gounelle and Siemens, than upon the calculation.

17. [VII. p. 118.]

It is not without interest to inquire how the interferences should have taken place if the experiments had led to the conclusion that the velocity in wires is equal to the velocity in air. This can easily be deduced from the correct theory given in No. IX., together with the aid of Fig. 31, and comes out as follows:—

	0		1		2		3		4		5		6		7		8
100	+	+	0	0	−	−	−	−	−	−	−	−	−	−	−	−	−
250	0	−	−	−	0	0	0	0	0	0	0	0	0	0	0	0	0
400	−	−	0	0	+	+	+	+	+	+	+	+	+	+	+	+	+

If the velocities were equal, there should still have been *one* change of sign; but the further changes which the experiments gave can only be explained by a difference in the velocities, or by illusions due to reflections or disturbances in the neighbourhood.

18. [VII. p. 131.]

It should be observed that we are here only able to determine the position of the magnetic force by the aid of theory. From the experiments we cannot conclude that a second kind of force is present together with the electric force. If we confine ourselves to the experiments, we can only regard the expression "magnetic force" as a short name for a certain mode of distribution of the electric force. That this magnetic force produces effects which cannot be explained by the electric force, is first verified by experiments in No. XII.; and, of course, only for waves in wires.

19. [VIII. p. 133.]

The wave-length measured depends, therefore, very much upon the distance of B and C; and hence upon the assumption that C is quite accurately measured. If we assume that the position of C is altered by general conditions of the surrounding space, the first node should be placed nearer to the wall and we might obtain much smaller values for the wave-length. But the experiments give no reason for believing that the position of C is uncertain.

20. [VIII. p. 136.]

Lloyd's experiment is the optical analogue of the experiments in which the primary conductor is gradually moved away from the wall. The experiments of the first kind, in which we removed the secondary conductor from a reflecting wall, have also found an optical analogue in the beautiful experiments which Herr O. Wiener has published in his paper on "Stationary Light-Waves and the Direction of Vibration of Polarised Light" (*Wied. Ann.* **40**, p. 303).

As to the acoustic analogues, I find that the phenomenon which forms the analogue to the experiments of the first kind was discovered by N. Savart many years ago (see *Pogg. Ann.* **46**, p. 458, 1839; also a number of Seebeck's paper in the subsequent volumes). If a steady source of sound is placed at a distance of 15-20 metres in front of a plane wall, and if we listen near the wall (best with the aid of a resonator), we find that the sound swells out at certain points—the antinodes,—and becomes weak at other points—the nodes. A correct analogue to the experiments of the second kind—in which the primary conductor is moved—has been already given in the text. Another analogue—in itself interesting—is the following. Take a glass tube about 60 cm. long and 2 cm. in diameter and lower it gradually over a Bunsen burner, of which the flame is not too large. At a given depth the Bunsen flame will begin, but not without some difficulty, to make the tube sing loudly. Now bring the system near to a wall. Quite near the wall the sound disappears; it

reappears at a distance of a quarter wave-length, and again vanishes at a distance of half a wave-length. By very careful adjustment, which up to the present I have not been able to secure at will, I have been able to observe two further positions of sound and silence at distances of half a wave-length. I do not know of any complete explanation of this phenomenon. Probably it has some connection with the fact that such a tube becomes silent if a resonator, tuned to the same note, is brought near its end. This last experiment is due—as far as I am aware—to Professor A. Christiani (*Verhandl. d. phys. Gesellsch. zu Berlin*, Dec. 15, 1882, at end of the *Fortschritte der Physik*, **36**).

21. [VIII. p. 136.]

This remark refers to the experiments with wires, which I was arranging at the time when this paper was written. It has already been stated in the Introduction that the hope here expressed has not been fulfilled.

22. [IX. p. 141.]

An error in sign in the original paper, to which M. L. de la Rive drew my attention, has here been corrected.

23. [IX. p. 150.]

This calculation is based upon the observed wave-length of 480 cm. If this is not correct, the calculation must be altered accordingly. With regard to the real value of the damping see Note 5.

24. [X. p. 161.]

By the experiments in the following paper it is pretty plainly proved that in the case of rapid variations of current the changes penetrate from without into the wire. It is thereby made probable that in the case of a steady current as well, the disturbance in the wire itself is not, as has hitherto been assumed, the cause of the phenomena in its neighbourhood; but that, on the contrary, the disturbances in the neighbourhood of the wire are the cause of the phenomena inside it.

That the disturbances in the wire are connected with a regular circulation of material particles, or of a fluid assumed *ad hoc*, is a hypothesis which is neither proved nor disproved by our experiments; they simply have nothing to do with it. We have neither any right to oppose this hypothesis, nor have we any intention of doing so, on the ground of the experiments here described.

25. [XI. p. 177.]

In connection with these phenomena we may refer to the observation which Herren Hagenbach and Zehnder have brought forward as an objection to my interpretation of the experiment (*Wied. Ann.* **43**, p. 611). My meaning is that light behaves just as the electric waves here behave; but we must imagine the dimensions of everything concerned in the experiment to

be reduced in the same proportion, not only the length of the waves.

26. [XI. p. 181.]

Herr W. König first pointed out that the analogy between the reflection of electric waves from our grating and the reflection of monochromatic light from the surface of dichroic crystals is much more complete than the analogy which is drawn in the text. He has also drawn attention to the relation between the action of our grating and certain polarising effects of optical gratings (*Wied. Ann.* **37**, p. 651, at the end).

27. [XI. p. 182.]

Messrs. Oliver Lodge and Howard have actually succeeded in showing the refraction and concentration of electric rays by means of large lenses (*Phil. Mag.* **27**, p. 48, 1889).

28. [XI. p. 185.]

Since then the experiments have been exhibited objectively in many ways. Herr R. Ritter has employed successfully a frog's leg (*Wied. Ann.* **40**, p. 53). Mr. Dragoumis has used Geissler tubes (*Nature*, **39**, p. 548). Herr Boltzmann has given a very convenient method in which a gold-leaf electroscope is used (*Wied. Ann.* **40**, p. 399). Herr Klemenčič has used a thermo-element (*Wied. Ann.* **42**, p. 416). The method which is most elegant and best adapted for demonstration, although it is far from being an easy one, is the bolometer method which Herren H. Rubens and R. Ritter have employed for exhibiting the experiments and for further useful researches (*Wied. Ann.* **40**, p. 55, and subsequent volumes).

29. [XIII. p. 198.]

And by more than one independent variable. The "force" and "polarisation" in this paper are not to be regarded as two variables in this sense; for they are connected by a fixed linear relation. If this relation is allowed to drop, by regarding it as a special case of a general relation, then "force" and "polarisation" may serve as two variables. But it would be more convenient to introduce the polarisation of the ether as the one variable, and the polarisation of the ponderable matter as the other.

30. [XIII. p. 214.]

According to this usual system of nomenclature it is undoubtedly true that the amount of "electricity" on an insulated sphere remains unchanged when the sphere is immersed in an insulating fluid, or, speaking generally, when it is moved in any way through insulating media. Hence we have denoted as "true" electricity the magnitude which remains unchanged during such motion. The distance-action of the sphere, and therefore the "free" electricity does change during the motion.

31. [XIII. p. 220.]

Consider a steam-engine which drives a dynamo by means of a

strap running to the dynamo and back, and which in turn works an arc lamp by means of a wire reaching to the lamp and back again. In ordinary language we say—and no exception need be taken to such a mode of expression—that the energy is transferred from the steam-engine by means of the strap to the dynamo, and from this again to the lamp by the wire. But is there any clear physical meaning in asserting that the energy travels from point to point along the stretched strap in a direction opposite to that in which the strap itself moves? And if not, can there be any more clear meaning in saying that the energy travels from point to point along the wires, or—as Poynting says—in the space between the wires? There are difficulties here which badly need clearing up.

32. [XIII. p. 221.]

In order to deduce the mechanical forces from the changes of energy, we must impart virtual displacements to the bodies. Hence we should have to use the equations for bodies in motion and not for bodies at rest, and at present the former are not at our disposal. By the aid of the experimental fact here assumed we are able to fill up this gap for the statical and steady states satisfactorily.

33. [XIV. p. 244.]

This proof that the statements here made embrace the observed facts, is also a proof of the statements themselves. They are therefore logically stated as facts derived from experience; not as results of any particular experiment, but as results of all the general experience which we possess respecting such matters.

34. [XIV. p. 246.]

The meaning of the equations is exceedingly simple; but their external appearance is somewhat complicated. This led me to expect that skilful mathematicians might be able to replace them by more elegant forms. And in fact Signor Vito Volterra has succeeded in representing by a single system of equations the phenomena for bodies both at rest and in motion (*Il nuovo Cimento* (3), **29**, p. 53; see also p. 147 *ibid.*).

35. [XIV. p. 255.]

A similar theory has also been developed recently by J. J. Thomson (*Phil. Mag.* (5), **31**, p. 149). In so far as this theory and Poynting's lead to Maxwell's equations, I would regard them as special forms of "Maxwell's theory," although their conceptions are undoubtedly not Maxwell's.

36. [XIV. p. 267.]

This does not necessarily imply an error in the theory, though it does necessarily imply a lack of completeness in it. Moreover it seems to be at the very root of our view, for it can be understood without using the equations. Let us suppose a magnetised steel sphere to rotate in free space about an axis which does not coincide with the direction of magnetisation. It continually sends out

electromagnetic waves; it therefore gives out energy and must gradually come to rest. Now let us take an iron sphere at rest and excite in it a rotary magnetisation by varying electric forces; it will easily be seen that the iron sphere must, conversely, begin to rotate. Such conclusions scarcely seem probable. But in connection with these matters we have scarcely any right to speak of probability,—so complete is our ignorance as to possible motions of the ether.

INDEX TO NAMES

Ampère, 230, 231
Bernstein, 269
Bezold, v. 2, 3, 54, 55
Biot, 230
Bjerknes, 17, 270
Blondlot, 12, 273
Boltzmann, 27, 276
Christiani, 275
Cohn, 28, 205, 273
Colley, 29
Cornu, 17
Coulomb, 225
Dragoumis, 276
Ebert, 94
Elster, 4
Engler, 103.
Faraday, 7, 19, 101, 122, 136
Feddersen, 29, 269
Fitzgerald, 3
Fizeau, 9, 114, 273
Fresnel, 136, 237, 240
Gauss, 138, 199, 231
Geissler, 32, 60
Geitel, 4
Gounelle, 114
Hagenbach, 17
Hallwachs, 4, 94, 272
Heaviside, 160, 196
Helmholtz, v., *passim*
Howard, 276
Jaumann, 271
Jochmann, 255
Joule, 219
Kirchhoff, 50, 261, 264, 269
Klemenčič, 276
Kolaček, 179
König, 276
Lecher, 12, 187, 188, 273
Lloyd, 136, 274
Lodge, 3, 161, 276
Lorenz, 50, 269
Maxwell, *passim*
Mosotti, 23
Neumann, 50, 232, 234, 237, 254
Newton, 136
Oberbeck, 42
Oettingen, v. 269
Paalzow, 269
Peltier, 219
Poincaré, 9, 17, 27, 271
Poisson, 23
Poynting, 139, 160, 220, 255, 277
Riess, 64
Righi, 4
Ritter, 186, 276
Rive, de la, 12, 13, 14, 16, 275
Röntgen, 95, 258
Rowland, 249, 259
Rubens, 186, 276
Rühmkorff, 31, 60, 64, 173
Sarasin, 12, 13, 14, 16
Savart, 59, 230, 274
Schiller, 269
Seebeck, 274
Siemens, 9, 114
Thomson, J. J., 273, 277
Thomson, Sir W., 50, 269
Trouton, 13
Volterra, 277
Weber, 51
Wiedemann, 94
Wiener, 274
Zehnder, 17, 275

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